VIEWS ON SCIENCE POLICY OF THE AMERICAN NOBEL LAUREATES FOR 1981

HEARING

BEFORE THE

COMMITTEE ON SCIENCE AND TECHNOLOGY U.S. HOUSE OF REPRESENTATIVES

NINETY-SEVENTH CONGRESS

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*On assignment to Budget Committee for 97th Congress.

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VIEWS ON SCIENCE POLICY OF THE AMERICAN NOBEL LAUREATES FOR 1981

THURSDAY, FEBRUARY 25, 1982

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
Washington, D.C.

The committee met, pursuant to notice, at 10:10 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua, the chairman of the committee, presiding.

Present: Representatives White, Lundine, Hollenbeck, Walker, Carney, Sensenbrenner, Weber, and Skeen.

Chairman FUQUA. I am very pleased to call to order this hearing with five distinguished American scientists who last December were awarded the Nobel Prize in Science.

Among the scientists from around the world who have been selected to receive the Nobel Prize, the record of American scientists has been outstanding.

Since the early sixties, the scientific contributions of U.S. scientists have clearly been of a caliber to merit the highest recognition by the Nobel Foundation, and, through the Nobel Foundation, the recognition of the world scientific community.

That record, I believe, is a clear reflection of the science policy that we have pursued in this country. Since the end of World War II, the Federal Government has been the major contributor to the funding of research and has sought to encourage methods of managing and administering that research in ways to encourage the best possible results.

The other major partner in carrying out that policy has been America's research universities. By providing an environment that permits the free selection and communication of research, and which encourages a continuing and close interaction between research and education, the universities have made possible the broad based, high quality science enterprise which the Nobel Prize awards reflect so well.

Today, however, that support and those policies have entered an era of uncertainty. The policy of significant annual funding growth is being questioned, and regulation of research activities is on the rise. Members of this committee and many of our colleagues in the Congress share our concern about the future direction of America's science. It is not unreasonable to ask if the policy changes now being suggested will substantially reduce the ability of American researchers to compete for the Nobel Prizes in 1990 and in the year 2000.

We look forward to hearing the views and recommendations of our witnesses. I have asked them to discuss Government science policy in general, or any aspects of science policy to which they feel we should pay particular attention.

I hope we can complete the presentations of the prepared statements in time to allow all members ample opportunity for questions and answers.

I welcome you here, and I extend a special welcome to those members of your families and your close friends who also join us today. We congratulate you and extend to you our warmest regards.

Our first witnesses, because Dr. Hanson is a physicist, will be the prizewinners in physics: Dr. Arthur L. Schawlow of Stanford University and Dr. Nicolaas Bloembergen of Harvard; we also have the prizewinner in chemistry, Dr. Roald Hoffman of Cornell. The prizewinners in medicine are Dr. Wiesel of the Harvard Medical School and Dr. Hubel, also from the Harvard Medical School. We are pleased to have all of you here. Dr. Schawlow, if you would like to begin.

Dr. SCHAWLOW. Perhaps it should be mentioned that the order of these people was specified in Nobel's will and ever since then it has been the traditional order in which Nobel Prize winners are presented, even by nonphysicists.

I have written some remarks, which I think I will skip over.

Chairman FUQUA. Let me say at the outset that, without objection from the members, we will make your prepared statement a part of the record in its entirety, and if you wish to summarize in any fashion we would be pleased for you to do that.

Dr. SCHAWLOW. Thank you, I would like to do that.

Chairman FUQUA. And, without objection, photographs and recording devices may be used in the course of the hearing.

[The biographical sketch of Dr. Schawlow follows:]

DR. ARTHUR LEONARD SCHAWLOW

Born In Mount Vernon, New York, May 5, 1921.
B.A.—University of Toronto, 1941.
M.A.—University of Toronto, 1942.
Ph.D.—University of Toronto, 1949.
Honorary Degrees; D.Sc., State University of Ghent, 1968; D.Sc., University of Bradford, 1979; and L.L.D., University of Toronto, 1970.
Demonstrator in Physics, University of Toronto, 1941-44, 1945-49.
Demonstrator in Microwave Development, Research Enterprises, Limited, 1944-45.
Fellow and Research Associate, Columbia University, 1949-51.
Research Physicist, Bell Telephone Laboratories, 1951-61.
Visiting Associate Professor, Columbia University, 1960.
Professor of Physics, Stanford University, 1961 to date, Chairman, Department of Physics, 1966-70, 1973-74.
Ballantine Medal, Franklin Institute, 1962.
Thomas Young Medal and Prize, British Institute on Physics, 1963.
Liebmann Memorial Prize, Institute of Electrical and Electronics Engineers, 1964.
Member: National Academy of Sciences, AAAS, American Physical Society (Fellow), Optical Society of America (Fellow; Chairman President in 1975), Institute of Electrical and Electronics Engineers (Fellow).
Research Interests: Radio frequency, optical and microwave spectroscopy, lasers and quantum electronics.
Nobel Prize winner in Physics, 1981.

BEST COPY AVAILABLE

STATEMENT OF DR. ARTHUR L. SCHAWLOW, JACKSON-WOOD PROFESSOR OF PHYSICS, STANFORD UNIVERSITY, PALO ALTO, CALIF.

Dr. SCHAWLOW. I have been privileged to watch the growth of lasers from the earliest search for ideas to the present large industry. When we started out, we were really just looking for some way to produce shorter wavelengths without having any definite objective or any in particular wave length or power level we had to reach. And, if we had had any such additional requirements, we might not have been able to do anything. It was really important that we were able to search freely in this rather broad direction. Then, when we did have the first lasers they were not much good for anything. There were dreams of drilling tunnels through mountains and so on, ancient dreams from science fiction, but much work had to be done before it could be used for anything. Although there were some surprises rather soon, such as the application to eye surgery. Now, lasers are used very extensively, proving to be more reliable than the machine tool they replaced in industry because there are no tool bits to wear.

Laser Focus Magazine recently estimated 1981 worldwide expenditures on lasers as $1.27 billion, of which $383 million was commercial. The laser industry makes a substantial contribution to this country's export trade. All that has grown from the ideas of many people and based on the earlier scientific discoveries.

The scientific applications are, of course, the thing that Bloembergen and I are especially interested in. They have made it possible for people to detect even a single atom of some substance, and that has profound implications A few years from now we may be able to detect one molecule of, say, a cancerous substance in a quart of milk. How would we regulate that. Or, we may find traces of helpful substances in very small amounts. They all raise questions but it is an exciting field and still moving very rapidly.

I do want to say a little bit about the problems and difficulties that I see confronting our fields of science and one is that you cannot do much research without money. This is increasingly true, particularly because we now have a very good scientific instrument industry. Whereas a generation or two ago we could build all our own equipment, you cannot really do competitive frontier research now unless you are able to buy equipment.

From my own experience, one example that I want to mention that really brought that home dates back to when I went to work at Bell Telephone Laboratories in 1951. We had lots of money for supplies. We could buy $1,000 worth of platinum and throw it away the next day if we had to, but we could not spend any money on capital equipment unless we could junk some old equipment. The reason was that the company was set up as a nonprofit subsidiary of the telephone company and if they added to their capital they were making a profit. Well, we struggled along and after about 5 years they suddenly realized that they could buy capital equipment if it was for a specific experiment and not for general use. And so the purse strings were loosened and we could get anything we needed.

My productivity shot up and so did everybody else's. Moreover, management, including Dr. William Baker, who is president of Bell Labs, absolutely agreed that there was a tremendous increase in research productivity when we could get the equipment we needed. We really had not realized what we were missing as a result of these restrictions. When equipment is obsolete it tends to lock old scientists into doing the same things over and over again and to shut our young scientists off.

We need young scientists to come into the universities. Some of my best students, who are offered university positions, will not take them since they feel that they cannot get any money to do anything there. If we do not find a way to get them started, and get some equipment, then we are shutting off the future.

Another thing I would like to say a few words about is the growing number of attempts to restrain scientific communication, allegedly in the name of national security, or sometimes to protect commercial leadership in high technology areas. There really does seem to be some confusion of these quite different objectives. But, the result appears to be a revival of the attempts, tried and abandoned as unwise in the 1950's, to prevent transmission of unclassified technical information. Now, as it was found then, what are really created are barriers for American scientists, cutting them off not only from foreign sources of information, but also from other Americans. These attempts are particularly untimely. As other advanced countries forge ahead in important areas of science and technology, we Americans have more than ever to discuss with our scientific counterparts.

Obviously real military secrets have to be protected, but when the net is cast so widely as to cover all facts that have any ultimate possibility of being useful, it catches many things that should be left alone.

Now, I want to say that during my own research I have often been surprised at how stupid I have been and how I have missed a lot of things that should have been plain and others found them. I really felt stupid. But, the only thing that saved me was that everybody else had been pretty stupid too. So, there really still are a lot of beautiful, simple things lying around to be discovered. To quote Vannevar Bush, "science is still the endless frontier."

One might ask, though, why Congress should bother with the expense and trouble of supporting science and protecting it from hampering restrictions? A good answer was given by the late Adm. Richard E. Byrd in his 1928 book "Skyways." He said:

> What then is the good of adding to man's store of abstract knowledge? The answer to this must come ultimately in material results, if at all. For example, as a result of centuries of apparently aimless research, we now have the telephone, the telegraph, radio, airplanes, anaesthesia, antitoxin, illuminating gas, electric light, X-rays, and automobiles, and a thousand other devices that make life safer and happier. Every one appeared to come suddenly and seemed to be the work of an inspired inventor. But that was not really true. Each was the culmination of generations of plodding abstract inquiry into the unknown, and more often than not the inquirer was jeered or feared for being a necromancer.

We could add a lot more to that list now. But in coming years we are going to need still more of these discoveries and inventions and they are only going to come if they are solidly based on the scientific research that we do now.

[The prepared statement of Dr. Schawlow follows:]

Remarks on Physics Research

A. L. Schawlow

I have been privileged to watch the growth of lasers from the earliest search for ideas to the present large industry. It has been intriguing to observe the effects of the uniquely cumulative nature of science. That is, many things that we began with a small insight or discovery have been turned into the foundations of large scientific and technological enterprises.

But when we began, Charles H. Townes and I were just trying to see whether the principle of the MASER, which he had initiated a few years earlier and which Nicolaas Bloembergen had advanced so importantly, could be extended from the radio region to the much shorter wavelengths of visible light. Fortunately, the optical properties of some kinds of atoms had been tabulated, most notably in the Bureau of Standards tables. Information about the lifetimes of excited atoms, and about how easy it might be to stimulate their emission, was scarcer. Nevertheless, there was enough so that we could calculate a simple example, and satisfy ourselves that some such device could be built. We also invented a new structure for the resonator of an optical maser or, as it is now known, laser.

For this investigation, we needed no reason other than the history of radio. We knew that whenever people had found ways to generate shorter wavelengths, those wavelengths had been found useful. Thus we were not constrained, other than to try and find a way to generate any wavelength in that region at any power level, small or large. If there had been any further restrictions, we might not have been able to do anything.

When we published our results in December 1958, some people doubted

that any such device could be made at all. Others believed that it might really be possible to make some kind of laser, though probably quite difficult. In mid 1960, Theodore H. Maiman announced successful operation of a ruby laser. By the end of that year, four other lasers were achieved, including the continuous-wave gas laser by Ali Javan, William Bennett and Donald Herriott. We were suprised to learn that making a laser was not difficult. Many different materials and methods of excitation could be used. Then people began to ask why lasers hadn't been made much sooner. Indeed they could have been, for all the necessary techniques were available, but the basic idea and concepts had been lacking. The fundamental laws of physics were known well enough, but nobody took seriously the possibility that those laws would permit us to make a device that worked so far from thermal equilibrium.

Once there were lasers, many people expected that they could solve some old problems, like drilling tunnels through mountains and shooting down Klingons a million miles away. After all, that sort of thing is an old dream of science fiction, such as H. G. Wells' "War Of The Worlds," or Buck Rogers in newspaper comics. But the reality was very different. The early lasers were very primitive, and couldn't do anything on a large scale. So much, much more had to be invested for research and development to find how to make lasers that would do useful tasks. By now, lasers of many kind are very widely used for industrial, medical and military applications. For manufacturing processes, lasers are proving to be often more reliable than the machine tools they replace, as there are no tool bits subject to wear. Laser Focus magazine recently estimated 1981 worldwide expenditures on lasers as $1.27 billion dollars,

of which $383 million was commercial. The laser industry makes a substantial contribution to this country's export trade.

But the applications that I find most satisfying are the scientific ones, because that is where the future technology will be found. It is rare now to open an issue of a journal in any branch of science without finding at least one report of work for which a laser was essential. Lasers have properties far more subtle than was envisioned in the old dreams of death rays. Their light can be extremely monochromatic, that is, of very exact single wavelength or frequency. To ordinary light, they compare as a pure musical note does to a noise. As ways have been found to make these lasers tunable, we have been able to use them in place of spectrographs, that is, for spectroscopy. We can scan a tunable laser across a band of wavelengths to determine at what wavelengths the light is absorbed. For instance, small semiconductor diode lasers are being used to test for pollutants in automobile exhausts. Other kinds of lasers can be tuned to just the wavelength needed to excite some particular kind of atom or molecule. Then from the scattered light, or by ionizing the atom, as little as one single atom can be detected. These methods are far more sensitive than even radioactive analysis. The atom is not destroyed by the light, and so we can scatter from a given atom as many light quanta as needed to detect it.

But the intense laser light does affect the substance that absorbs it. Chemical reactions, in general, go faster when substances are heated. Lasers provide highly selective ways to heat things up. Thus we can tune lasers so that their light is absorbed, for instance, by one isotope and not by another isotope of the same element. This is the

basis for laser separation of isotopes, such as those of uranium. It may well be that laser isotope separation will be important for nuclear power systems in the 1990s. On the other hand, if some easy way to use lasers for separating uranium isotopes were to be found, it might well add to the dangers of weapons proliferation. Fortunately, so far that seems to be quite difficult.

During earlier years, the use of spectrographs to analyze the light emitted and absorbed by atoms and molecules has added immeasurably to our knowledge of the universe and of the fundamental physical laws that govern it. Without this understanding, it would not be possible to conceive such devices as lasers, transistors and integrated circuits, and modern life would be much poorer. Yet, as an English scientist writing in Nature put it, "In the past fifteen years spectroscopy has ceased to be simply a powerful but established technique, in some ways changing technically more than at any time since Newton." Ways have been found to reveal fine details in the spectra of atoms and molecules, whose existence could previously be only suspected because they were blurred by thermal motions. Measurements are being made with enormously improved precision, making possible new tests of some of the fundamental laws of physics. We are now able to use lasers to simplify complicated spectra, so as to understand better how those laws work to build the complex atoms and molecules that make up much of matter.

Many outstanding scientists are contributing discoveries to this exciting field. The tools are being created which make it possible to look at things much more deeply than we could ever do before. I confidently expect some surprising and important results from these

activities within the next decade or so.

But there are problems and difficulties that may very well seriously slow progress in the next few years. You can't do much research without money, and this is increasingly true. The scientific instrument industry, especially strong in this country, offers a variety of powerful tools which one needs to be at the frontier of research. A generation ago, one might build all of his own apparatus, but to do so nowadays is to nearly always lag behind the advancing frontiers of science. Of course, an ingenious scientist can always do something by improvising with what little is available. That was the way things were when I first went to Bell Telephone Laboratories in 1951. We could have all the money we wanted for supplies, but the purchase of new equipment would add to the Laboratories' capitalization and could only be done if old equipment of equivalent price were retired. Then, about half way through my ten years at the laboratory, a change in policy made it possible to acquire essentially whatever equipment was needed for a proposed experiment. I found that my research productivity, and that of my colleagues, increased dramatically. We simply had not realized how much time we had been wasting in trying to work around the limitations of obsolete or inadequate equipment.

Now it is widely recognized that the research equipment available to many American scientists is really obsolete, and not keeping up with that available in some other advanced countries. For established scientists, this tends to force them to continue repeating variations of their older work, rather than to undertake something really new. But for young scientists, especially at universities, obtaining equipment funds to

begin research can be a nearly insuperable obstacle. Some very good young scientists are choosing not to join university faculties, even when positions are available, because they fear that they will not be able to do any research. The universities need to attract such people, for they are the ones who will be, and will educate, the scientists of the future. It is important to note, in these days when the problems of engineering manpower and education are being recognized, that physicists play an important part in new technologies. In 1960, there were no trained laser engineers, and physicists predominated in the development of lasers. Similarly with the sudden growth of genetic engineering, much of the work is being done by people trained as scientists. It is important that the universities continue to be staffed by creative people who are themselves participating in the creation of new knowledge. These are the people who can teach science as a dynamic, growing subject rather than as a body of dusty facts entombed in textbooks.

Another matter of concern is the growing number of attempts to restrain scientific communication, allegedly in the name of national security, or sometimes to protect commercial leadership in high technology areas. There really does seem to be some confusion of these quite different objectives. But the result appears to be a revival of the attempts, tried and abandoned as unwise in the 1950s, to prevent transmission of unclassified technical information. Now, as it was found then, what are really created are barriers between American scientists, cutting them off not only from foreign sources of information, but also from other Americans. The attempts are particularly untimely, as we have more than ever to learn from discussions with other advanced countries,

as they are forging ahead in some areas of science and technology.

Obviously real military secrets must be restricted. But when the net is cast so widely as to try to keep all facts having any ultimate possibility of being useful, it catches many things that should be left alone. Some proposed regulations would prevent publication for international distribution of anything not either specially licensed or already published. Now, some of our scientists do want to publish things that have already been published, but that is not the normal procedure. The topics discussed at open meetings are being limited, and in at least one case, officials of a scientific society have been ordered to exclude foreigners whose presence had already been approved by officials of another government department. Universities have been told to limit enrollments in some classes to American citizens, and to restrict the activities of foreign visitors. Much of this is, I believe, misguided because the kinds of facts discussed at scientific meetings and in classrooms are the basic principles of science or of devices. Actual manufacturing details are closely guarded by the companies for commercial reasons. Thus the effect of restrictions is to reduce the flow of information and ideas to creative American scientists and technologists, and ultimately thereby to reduce this country's technological lead.

During my scientific research, I have often been surprised by others discovering simple and obvious things that I should have, but did not see. I have often been short sighted and even felt rather stupid. But the thing that has saved me is that everyone else has been too. Thus, I am certain that there are many more simple and beautiful things to be found in science. It is still, in Vannevar Bush's term "The Endless Frontier."

But why should congress bother with the expense and trouble of supporting science and protecting it from hampering restrictions? A good answer was given by the late Admiral Richard E. Bird in his 1928 book "Skyways." He said:

> "What then is the good of adding to man's store of abstract knowledge? The answer to this must come ultimately in material results, if at all. For example, as a result of centuries of apparently aimless research we now have the telephone, the telegraph, radio, airplanes, anaesthesia, antitoxin, illuminating gas, electric light, X-rays, and automobiles, and a thousand other devices that make life safer and happier. Every one appeared to come suddenly and seemed to be the work of an inspired inventor. But that was not really true. Each was the culmination of generations of plodding abstract inquiry into the unknown, and more often than not the inquirer was jeered or feared for being a necromancer."

We could add a lot more to that list now. But in coming years we are going to need still more. Thus it is important to continue to provide the conditions under which new discoveries will be made, and to help promising young scientists get started. Above all, we must remember that we are looking for surprises, and that we can usually not expect to see their potential uses at all.

A. L. Schawlow
Department of Physics
Stanford University
February 25, 1982

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Chairman FUQUA. Thank you very much, Dr. Schawlow. Dr. Bloembergen?

[The biographical sketch of Dr. Bloembergen follows:]

BIOGRAPHICAL DATA

Name in full: Nicolaas Bloembergen

Date and place of birth and citizenship: March 11, 1920, Dordrecht, The Netherlands
Naturalized U.S. Citizen, April 28, 1958

Academic degrees:		
	Phil. Cand.	1941, University of Utrecht
	Phil. Drs.	1943, University of Utrecht
	Ph.D.	1948, University of Leiden
	A.M. hon.	1951, Harvard University

Positions held:

Teaching Fellow (part-time), University of Utrecht, 1942-1945
Research Assistant (part-time), Harvard University, 1946-1947
Research Associate, University of Leiden, 1947-1948
Society of Fellows, Harvard University, 1949-1951
Associate Professor of Applied Physics, Harvard University, July 1951-1957
Gordon McKay Professor of Applied Physics, Harvard University, 1957-present
Rumford Professor of Physics, Harvard University, 1974-1980
Gerhard Gade University Professor, Harvard University, 1980-present

Honors:

Member of the National Academy of Sciences, Washington, D.C.
Fellow of the American Academy of Arts and Sciences, Cambridge, Massachusetts
Correspondent of the Koninklyke Nederlandse Akademie van Wetenschappen, Amsterdam
Fellow of the American Physical Society, Dutch Physical Society
Fellow of the Institute of Electrical and Electronics Engineers
Fellow of the Optical Society of America
Honorary Fellow, Indian Academy of Sciences, Bangalore, India
Associé Étranger de l'Académie des Sciences, Paris

Oliver E. Buckley Prize for Solid State Physics, 1958. Awarded for outstanding contribution in the field of magnetic resonance, both nuclear and electronic, and for applications of this technique in solids, liquids, and gases.

Morris Liebmann Memorial Award of the Institute of Radio Engineers, 1959 (with Professor C. H. Townes, Columbia University). Given in recognition of contributions to the maser art.

Stuart Ballantine Medal of the Franklin Institute, 1961 (with H. E. D. Scovil) for the invention and subsequent development of the three level solid state maser.

The National Medal of Science, 1974, awarded by the President of the United States "For pioneering applications of magnetic resonance to the study of condensed matter and for subsequent scientific investigations and investigations concerning the interaction of matter with coherent radiation."

Lorentz Medal, 1978, awarded by the Royal Dutch Academy of Sciences, Amsterdam

Frederic Ives Medal, 1979, awarded by the Optical Society of America, "For establishing the theoretical framework of nonlinear optics and for sustained innovative contributions to the exploration of all aspects of nonlinear optical phenomena."

Nobel Prize for Physics, 1981, awarded by the Royal Swedish Academy of Sciences. (One half of the prize was awarded jointly to N. Bloembergen and A. L. Schawlow for their contributions to the development of laser spectroscopy. The other half was awarded to K. Siegbahn for his contribution to the development of high resolution electron spectroscopy.)

Half-Moon Trophy of the Netherlands Club of New York, 1972, in recognition of an outstanding Dutch immigrant.

Guggenheim Fellow 1957, Visiting Professor at the Ecole Normale Superieure in Paris, 1957.
Visiting Professor, University of California, Berkeley, 1965.
Lorentz Guest Professor, University of Leiden, 1973.
Raman Visiting Professor, Bangalore, India, 1979.
Alexander von Humboldt Senior U.S. Scientist Award, Munich, 1980.
Visiting Professor, Collège de France, Paris, 1980.

Consulting:

Schlumberger Well Surveying Corporation
Lincoln Laboratory, Massachusetts Institute of Technology
Perkin Elmer Corporation
Institute of Defense Analyses
National Science Foundation
United Aircraft Research Laboratories
Philips Research Laboratories
Itek Corporation: Chairman of Science Board; Director, 1972-1977
Energy Research and Development Agency (formerly AEC)
United Technologies Research Center

Director, E. Fermi Course on Nonlinear Spectroscopy, Varenna, Italy, 1975, and editor of the Proceedings.

Member, Joint Council for Quantum Electronics. Visiting Committee of the National Magnet Laboratory, 1966-1968; Visiting Committee of the Physics Department of the Brookhaven National Laboratory, the University of Rochester and New York State University at Buffalo; NASA Electrophysics Advisory Committee; ARPA Materials Research Council.

Lecturer at the International Summer Schools, Boulder 1962, Varenna 1963, Les Houches 1964, Vancouver 1966, Crete 1968, Copenhagen 1969, Edinburgh 1969, Buenos Aires 1978.

Associate Editor of the Physical Review, 1956-1958; the Journal of Chemical Physics, 1960-1962; the Journal of Applied Physics, 1964-1967; Institute of Electrical and Electronics Engineers Journal of Quantum Electronics, 1965-1970.
Advisory Editor of Optics Communications, 1969-present; Physica (Amsterdam), 1975-present; Nuovo Cimento, 1979-present.

His bibliography includes two books,

Nuclear Magnetic Relaxation, Benjamin, 1961
Nonlinear Optics, Benjamin, 1965

and about two hundred fifty papers in scientific journals on topics in Nonlinear Optics, Quantum Electronics, Solid State Masers and Magnetic Resonance.

STATEMENT OF DR. NICOLAAS BLOEMBERGEN, HARVARD UNIVERSITY, CAMBRIDGE, MASS.

Dr. BLOEMBERGEN. Mr. Chairman, I appreciate the opportunity to appear before you as an individual scientist. My brief remarks are not based on any extensive administrative experience, but merely reflect some individual observations and concerns of a scientist.

I was struck, however, by your introductory remarks, the remarks of my colleague, Schawlow, and conversations at breakfast with members of your committee, that, even so, I seem to be in touch and we are all concerned with similar issues.

If judged by its share of recent Nobel awards, the state of science in the United States may be labeled as excellent. One should, however, keep in mind that the groundwork for these successes was laid two or three decades ago. Some very wise and farsighted research policies were adopted in the years immediately following World War II.

I wish to take this opportunity to express my indebtedness to the joint services electronics program of the United States Department of Defense for support of my research over a period of more than three decades.

Although initially my research was concerned with a rather academic subject, gold magnetic resonance and relaxation, it led to the development of masers, used in sensitive radar installations, and it helped subsequently in the development of many laser applications about which my colleague, Schawlow, has spoken. Two points about basic research are illustrated by this example:

One, there is usually no short-term payoff; assessment of funding for basic research should be based on broad, long-term, strategic considerations.

Two, the characteristic leadtimes are long indeed. The training of a future scientist takes 10 years after graduation from high school. A typical research project for an experimental Ph. D. thesis takes 3 or 4 years.

The annual Ph. D. production in the United States reached a maximum of 1,740 in the year 1970, 10 years after the adoption of postsputnik policies in science education. In 1980 this annual production in physics Ph. D.'s had fallen to about half; 983. The current policies of decreased fellowship and loan support for undergraduates, as well as for graduate students, are perceived by high school and college students as an indication that national policy attaches a relatively low priority to science and engineering education. If these trends continue, a serious shortage of scientists and engineers may develop by the year 1990.

In times of economic stringency an individual is tempted to cut down on his life insurance. Similarly, industrial organizations often effect savings by cutting back on basic research and development. This may insure short-term financial viability, but the long-term consequences for high technology enterprises may be severe.

The national interest will not be served if agencies of the U.S. Government adopt similar procedures. In order to achieve economic and military preparedness in the 1990's, basic research now should be encouraged.

About half of the total basic research effort in the United States is carried out in universities, both public and private. Again, roughly one-half of this academic research, or one-quarter of the total basic research in the United States is funded in the form of small individual grants and contracts, of less than $150,000 each, annually. This support comes, for about two-thirds, from various U.S. Government agencies. This pluralistic system has functioned well during the past decades, and has sometimes been a source of envy for our colleagues in Western Europe. The advantages of the system are as follows:

One, funding is subject to periodic review by the agencies and scientific peer groups, and is awarded on a competitive basis.

Two, it permits funding of meritorious individuals without geographic or institutional constraints or preferences.

Three, it affords special opportunities for independence of young scientists. It permits freedom of movement and encourages a quest for scientific excellence in a manner which is similar to the encouragement of athletic competition in sporting events.

Four, it permits the rather quick exploitation of new opportunities.

This system has come under stress because the rapidly escalating costs of instrumentation and technical support for modern research have encouraged a trend toward larger, and often less flexible, research institutes.

The small-scale basic research units in academic institutions in the United States suffer from obsolescent instrumentation and support facilities. These are decidedly inferior, on the average, to those found at industrial and governmental laboratories in the United States, and also much inferior to those found in academic institutions on the Western European continent.

In my opinion it is worthwhile to keep the individual academic research projects competitive by providing special funds for a much-needed upgrading of instrumentation and support facilities in the universities of the United States.

Although the period of U.S. predominance in basic research in the physical sciences will probably come to a gradual ending, the outlook is that the United States can certainly remain competitive in basic research until the end of this century if the amount of support does not fall below the levels now prevailing in Western Europe, Japan, and the Soviet Union.

I thank you for your attention.

Chairman FUQUA. Thank you very much, Dr. Bloembergen. Your written testimony will be inserted into the record.

Dr. Hoffman?

[The prepared statement of Dr. Bloembergen follows:]

Statement to be read at the hearing of the Committee on Science and Technology, U.S. House of Representatives, on February 25, 1982

by

Nicolaas Bloembergen

Professor of Applied Physics, Harvard University

(Dr. Bloembergen, whose biographical data are appended to this text, represents himself.)

Mr. Chairman, Members of the Committee on Science and Technology:

The opportunity to appear before you as an individual scientist is much appreciated. You have recently heard testimony from several leaders in the scientific community with substantial expertise in issues of public policy. My brief remarks are not based on extensive administrative experience, but merely reflect some individual observations and concerns of a scientist-citizen

If judged by its share of recent Nobel awards, the state of science in the United States may be labeled as excellent. One should, however, keep in mind that the groundwork for these successes was laid two or three decades ago. Some very wise and far-sighted research policies were adopted in the years immediately following World War II. I take this opportunity to express my indebtedness to the Joint Services Electronics Program of the U.S. Department of Defense for support of my research over a period of more than three decades. Although initially my research was concerned with the rather academic subject of magnetic resonance and relaxation, it led to the development of masers, used in sensitive radar installations, and it helped subsequently in the development of many laser applications. Two points about basic research are illustrated by this example:

1. There is usually no short-term payoff; assessment of funding for basic research should be based on broad, long-term, strategic considerations.
2. The characteristic lead times are long. The training of a future scientist takes ten years after graduation from high school. A typical research project for an experimental Ph.D. thesis takes three or four years.

The annual Ph.D. production in physics reached a maximum of 1,740 in 1970, ten years after the adoption of post-sputnik policies in science education. In 1980 the annual production in physics had fallen to 983, thus down almost by a factor of two from that of ten years earlier. The current policies of decreased followship and loan support for undergraduates as well as graduates are perceived by high school and college students as an indication that national policy attaches a relatively low priority to science and engineering education. If these trends continue, a serious shortage of scientists and engineers may develop by the year 1990 (see references 1 and 2).

In times of economic stringency an individual is tempted to cut down on life insurance. Similarly, industrial organizations often effect savings by cutting back on basic research and development. This may insure short-term financial viability, but the long-term consequences for high technology enterprises may be severe. The national interest will not be served if agencies of the U.S. government adopt similar procedures. In order to achieve economic and military preparedness in the 1990's, basic research now should be encouraged. Assuming a fixed budgetary total for R and D expenditures, it would only require a very small percentage decrease in the much larger development budget, shifted to maintain a healthy level of basic research.

About half of the total basic research effort in the United States is carried out in universities, both public and private. Again roughly one half of this academic research, or one quarter of the U.S. total, is funded in the form of small individual contracts and grants, of less than $150,000 each annually (see references 3-5). This support comes, for about two thirds, from various U.S. government agencies. This pluralistic system has functioned well during the past decades, and has sometimes been a source of envy for our colleagues in Western Europe. The advantages of the system are:

1. Funding is subject to periodic review by the agencies and scientific peer groups, and is awarded on a competitive basis.
2. It permits funding of meritorious individuals without geographic or institutional constraints.
3. It affords special opportunities for independence of young scientists. It permits freedom of movement and encourages a quest for scientific excellence in a manner which is similar to the encouragement of athletic competition in sporting events.
4. It permits the rather quick exploitation of new opportunities.

This system has come under stress because the rapidly escalating costs of instrumentation and technical support for modern research have encouraged a trend toward larger, and often less flexible, research institutes. The small-scale basic research units in academic institutions in the United States suffer from obsolescent instrumentation and support facilities. These are decidedly inferior, on the average, to those found at industrial and governmental laboratories in the United States, and also inferior to those found in academic institutions on the Western European continent. In my opinion it is worthwhile to keep the individual academic research

projects competitive by providing special funds for a much-needed upgrading of instrumentation and support facilities in U.S. universities.

Although the period of U.S. predominance in basic research in the physical sciences will probably come to a gradual ending, the outlook is that the United States can certainly remain competitive in basic research until the end of this century, if the amount of support does not fall below the levels now prevailing in Western Europe, Japan and the Soviet Union.

I thank you for your attention.

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[The biographical sketch of Dr. Hoffmann follows:]

DR. ROALD HOFFMANN

Roald Hoffmann was born in Zloczow, Poland, in 1937. Having survived the Nazi occupation, he arrived in the U.S. in 1949 after several years of post-war wandering in Europe. He graduated from Stuyvesant High School, Columbia University, and proceeded to take his Ph.D. in 1962 at Harvard University, working with W. N. Lipscomb and Martin Gouterman. Dr. Hoffmann stayed on at Harvard University from 1962–1965 as a Junior Fellow in the Society of Fellows. Since 1965 he has been at Cornell University, where he is now the John A. Newman Professor of Physical Science.

Professor Hoffmann, a member of the National Academy of Sciences and the American Academy of Arts and Sciences, has been the recipient of the American Chemical Society's Award in Pure Chemistry. He is the only person ever to have received that society's awards in two different subfields of chemistry—the A. C. Cope Award in Organic Chemistry and the Award in Inorganic Chemistry. In 1981 he shared the Nobel Prize in Chemistry with Kenichi Fukui. Dr. Hoffmann holds honorary degrees from the Royal Institute of Technology and Yale University.

Roald Hoffmann is an applied theoretical chemist. He has used and developed a variety of computational procedures, as well as qualitative symmetry and bonding arguments, and has applied these methods to problems of structure and reactivity of both organic and inorganic molecules.

STATEMENT OF DR. ROALD HOFFMANN, PROFESSOR OF CHEMISTRY, CORNELL UNIVERSITY, ITHACA, N.Y.

Dr. HOFFMANN. It gives me special pleasure to appear here before you, committee members, 3 days after the 33d anniversary of my coming to the United States. I am a member of the last generation of Hitler's gifts to America and this country has been generous to me as it has been to generations of immigrants before me. It has taken a great deal of support, that of my parents, my family, great teachers, and great universities to get me both to Stockholm and subsequently here. It has also taken a great deal of institutionalized science support to get me here. I am especially grateful to this committee, as one part of that mechanism, that has assured me the support that I have needed in order to get my work done.

In my own particular case, for instance, all my support, my entire support over the years, has come from the National Science Foundation, and I am grateful to the committee for its continued support of the budgetary base for the National Science Foundation and also in its efforts to restore science education activities.

There are a number of issues that I could discuss with you. Some of these have already been mentioned by my colleagues: The need for a steady, unfluctuating, stable base of science support; the need not to be pressed too much to find short-range applications for our work; and our concerns with the imposition of secrecy requirements. But, I would like to tell you specifically of three matters that concern me. One of them has already been mentioned, science education. Another one is the interaction between universities and industry and the mediating role that government can play in those. The third thing has to do with our scientific relationship with the Soviet bloc.

In a field such as chemistry, the first two, science education, and science and universities and industry, are inextricably related with each other. The facts are very simple. In a field such as mine, chemistry, 70 percent of the Ph. D.'s, the product that we put out, of the 1,500 Ph. D.'s that we have per year in American universi-

ties, end up working for industry. So, we are providing the laborforce of the most highly skilled component of the labor force of American industry. So, obviously, there are common interests. These people are doing research with us and are going on to work in industry.

I am sorry to say that the great American industry, for all of its accomplishments, has done very little, except what it has done through paying its taxes, for the support of education in the universities. The facts are very simple. In a university such as Cornell, or pick any university in the country, less than 5 percent of the support of that university for education or research comes from industry. A lot of it comes from government. A lot comes from donations, through endowments to universities—some of it through industrial sources, to be sure—but they are really made on an individual, typical, American philanthropic pattern, which is a great thing but not an institutionalized mechanism for giving money to us.

There are some signs of increase in that support from industry to universities and I will say a word about that in a moment. But, let me turn to another concern about science education.

I think all of you and all of us have heard that young people are attracted less to the pure sciences—chemistry, physics, mathematics—than they used to be. There are many reasons for that. I have outlined in my written testimony several reasons and let me just outline what people say. First, there is a growing antirationalist mood in society evidenced by things like the occult, astrology, and so forth. Second, there is a loss of perception of science as being purely progressive, as leading to the betterment of mankind. When we have to deal with the problems of pollution in the environment, it is inevitable that science does not have that clean, white image that it had once upon a time. Third, there are meager financial incentives to a scientific career. Fourth, there is a deterioration in scientific literacy. And, there is a fifth factor which is a little difficult to define—maybe a psychologist can define it better than I—that there is an inevitable loss of infectious enthusiasm that had to come when the dynamic exponential growth of scientists had to season the century. You cannot run a society with scientists only, and the number of scientists growing had to stop. It stopped around 1970. When it stopped, inevitably when you level off, something happens to the psychology in the field—you do not feel so enthusiastic about getting into a field that is leveling off.

Now, given that you have all of these things, and we are agreed—and that goes for industry, the government, and us—that we want to get more people into research—into science—that is hardly the time to cut science education funds. What we need is more things to lure—I would rather use the word "attract"—people to what we are doing; to show them the excitement of research.

Those things come about in many ways. They come about through NOVA programs, such as "Cosmos" on TV. I think my colleague, Carl Sagan, has done more for science education than most other things around. They come through some of our research programs at universities; they come from new textbooks. It seems that teachers in high school need something every 20 years to revive their interest even if they are teaching the same thing in the high

schools. New programs are what do it. And, for all this, we need science education funds.

I mentioned something, ruefully, the poor record of industry in supporting research and education in my field. There are signs that times are changing. I see in my own department—I happen to be chairman of the chemistry department—signs of increasing interest, of increasing self-interest on the part of industry. After all, if we are not producing the Ph. D.'s, they are not getting their work force. So, we are seeing an inflow of funds, especially for graduate student fellowships and for support of young faculty members—a crucial need that we have—from industry and our interests are not at all divergent.

I would like you to think, because I cannot think, about some mechanisms for encouraging the inflow of funds from industry to the universities. I do not know exactly how to do it, but I think there is expertise among you to provide that additional inflow.

I would like to name one specific opportunity where universities and industry can interact. I am going to, with your permission, to submit for the record an extended document written by a colleague of mine on meeting the Japanese challenge in large-scale computing. I will say a few words about it, but the colleague of mine is a rather unusual person who is not in the computing field at all. It happens to be Kenneth Wilson and I have no doubt that in the next few years you will see him sitting at this table before you. He is an unusual young physicist at Cornell and he has become very concerned about the so-called Japanese plan for the fifth-generation computer.

Now, the normal computers that we have around and are accessible to most of us are so-called third generation computers. The fourth generation computers are these Crays—the things that only national laboratories are lucky enough to have and that we at universities do not have, and I am sure they come before you asking for 20 of them from time to time.

There is, now, a growing Japanese effort to produce a fifth-generation computer. This will be a computer that will be 1,000 times as powerful as one of these Crays. This is a very severe challenge to the United States and there are several barriers to U.S. innovation that could prevent a U.S. response.

One of them is that there are market requirements in the United States and market forces which mitigate against this. The market requirements are those of having the software—the programs that make possible the running of the computer—compatible. Everyone in the United States wants everything to work immediately and they want to carry over from the older computers to the new computers the software.

As you have also perceived, everyone in the United States is interested not only in having a super computer, but also their own little desk top computer to play with. But, someone has got to think about the bigger computers which keep our industry and science running and those are not little Apple computers—they keep one company running and a lot of people happy, but they do not keep universities running.

There is a law of diminishing returns also at work on computers that an improvement by a factor of 2 does not get you very much. You need an improvement by a factor of 100 or 1,000.

There is also a necessity, which we do not seem to have, of planning software systems before a market for these systems becomes evident. And, the most serious part of the Japanese threat is that they will accumulate software targeted for their fifth-generation computers—especially for industrial computer automation for which, as you know, they have very good contributions in the area of robot control of industry—and this is tied up to that.

Whereas the U.S. software development is restricted to existing systems with far less capability, there is a way to overcome these barriers which Ken Wilson suggests, and he has some experience with making such a system work at Cornell. It involves an industrial-academic interaction. It turns out that universities are the best places to test computers, not your local banks, which do not want systems that mess up all your accounts. Your universities are usually much more willing to take more hassle in making the thing work than your industrial institution which has all kinds of regulations around it.

We have some experience at Cornell with making a complicated computer system work in various stages of development. So, if a company pushes a computer to a certain point, let us try it. We then complain as hard as we can, the system gets improved and eventually it evolves into something marketable.

We have got to have the money to buy those computers and Ken Wilson has a novel idea, analogous to the potato board of the Department of Agriculture. His idea is a use tax, essentially on the sales of all computers, whether domestic or foreign, in the United States—and you can read more about this in the document—to finance the joint buying and development, between industry and university, of computer systems.

You can read more about this novel idea, or find out about the potato board. As I had to find out, it takes some time.

Now, what I want to say finally to you is something which is not terribly popular and that is something on the curtailment of scientific exchanges with the Soviet Union and its allies.

Let me tell you a little bit about my own background, because in dealing with questions which involve Eastern countries, one has to say something about what one knows or where one comes from. I was born in Poland. We had the fortune of Soviet occupation from 1939 to 1941 and from 1944 to 1946. It was a relative fortune only, to be exceeded only by the excesses of the German occupation from 1941 to 1944. I do not remember how bad things were under Soviet occupation because I was a little child at the time, but I remember from what my parents told me.

I went to the Soviet Union in 1960 for a year on an exchange sponsored by the State Department and I am probably one of the few people in this world who can give a lecture in the Soviet Union on my own work in Russian, as I have just done this past December.

In my own very international group at Cornell University, over the years I have had working with me, three Romanians, two Czechs, one Hungarian, one East German, and two young men—

very talented young men—from the People's Republic of China. So, I have some knowledge of the area.

In recent years, scientific exchanges with the Soviet Union have been curtailed for a variety of reasons. The Carter administration did it, with the consent of Congress, over Afghanistan. The current administration is doing it with great popular support over Poland. The Academy of Sciences has done it because of the treatment of a colleague of ours, Sakharov. Individual groups of scientists around the country have banded together to apply great pressure on other scientists to avoid all contacts with the Soviets because of yet a fourth reason: The treatment of refusniks, dissenters, and others who have been repressed in the Soviet Union.

What it all adds up to is a decimation of visits and exchanges in both directions. Let me tell you that I think that a serious mistake is being made here, and for a number of reasons. I think that despite the actions of the Soviets and their allies, actions that you and I disapprove of, that it is important to maintain contact between scientists of these countries, and that it is we as much as they who are hurt by the present situation.

Let me tell you a number of reasons why it is important to maintain contact. In that closed society, which is the Soviet Union, every small window that is opened up on the world brings the light in there and makes friends for us. Soviet scientists, the Soviet intelligentsia—that is a Russian word that has crept into the American language—which has been the most progressive segment of their society and remains such in these times, is the only segment of society which has an opening to the West, a sense of being with us, and, that sense of being with us is being reinforced by a visit by any American there, by any visit by a Russian here.

There is no question that every such visit, in either direction, has an official role. There is no question that the Russians will use our visits there, as they used mine in December of last year, for their own purposes. But, there is similarly no way in which they can shut off the personal side of a visit. Every time that I go there and I tell them about some exciting work that I can do and which they cannot do, every time I show them a picture of my laboratory at Cornell University, which you can be sure I do show them when I go, I open up a little bit of a window on the world which is extremely important and keeps us in touch and talking to them.

I would also argue that on simple self-interest grounds, the national security of this country is not well served by not having people here who have been there, who can travel there.

Finally, and with respect to that let me just say that I would urge you to seek the experience of Dr. Armand Hammer of Occidental Petroleum, who has made a very clear example of how you can deal with the Russians in a businesslike manner to our benefit. I do not think it is served by closing off communication.

The final point is that I think scientists have a responsibility, based on the rational and open tradition of their activities, to keep talking to each other even when the rest of society is disposed to get angry. It is not that we are better people. I think we just have a certain base of small talk, shop talk, and whether it is a relationship between Democrats and Republicans, or supporters of two rival football teams, or parents and child, you have got to keep a

base of small talk even when you are angry with each other so that when you stop being angry you have something to retrieve from. I think we are in that position to keep talking and I think it extremely important that since we have gotten ourselves into various squabbles on other points that we keep the scientific lines of communication open.

Thank you very much.

Chairman FUQUA. Thank you, Dr. Hoffmann. We will insert your written testimony and the document by Ken Wilson into the record at this point.

[Dr. Hoffman's prepared statement and supplemental materials for the record follow:]

Testimony of Roald Hoffmann
at Hearing of Committee on Science and Technology
U.S. House of Representatives
February 25, 1982

It gives me special pleasure to appear before you, Committee members, three days after the thirty-third anniversary of my arrival in the United States. This country was generous to me, a child of the Holocaust, as it has been to generations of immigrants. The efforts of many people combined to make it possible for me as an American to attain the Nobel Prize—the support of parents, my wife and children, great teachers and great universities. That I have been able to accomplish what I have is also due to the basic sympathy of American society to science and innovation, and to a climate of support for science that made my work possible. I mention this here because it is your Committee which through hard informed work and tough decisions has shaped the governmental mechanisms of support that have allowed me and my most international group of co-workers to do our work. Your specific support of the budgetary base for the National Science Foundation, my own sole source of research support for the last ten years, and your efforts to restore funds for science education activities are much appreciated.

There are a number of issues that I could discuss with you, that concern my colleagues and me. We are worried by fluctuations and lack of stability in support of basic research; by undue pressures to find short range applications for our

research; by needs of our scientific community for support of instrumentation; we are concerned with possible imposition of secrecy requirements on some basic research. But I would like here to express my ideas to you on two issues in some detail. These are science education, and our scientific relationships with the Soviet bloc.

In a field such as chemistry 70% of the Ph.D. graduates enter industry. Thus we train the highly skilled work force that is behind medicines, plastics, photography, synthetic fibers—indeed, every aspect of modern society, everything around you in this room. We and industry share a common interest in attracting talented young men and women to our profession. I'm sorry to say that the great American industry has not done its share in this training process—in a typical good department such as Cornell's less than 5% of our teaching and research support is from industry. Much support is from one or another government source. A substantial portion of the support comes from the gifts of our generous alumni and general university funds. There are industrial givers to be sure, but it is mainly individuals who support us. There are signs of increased industrial support, and I'll return to this in a moment.

Young people are not as attracted to science as they once were. Why? Several reasons come to mind: (1) A growing antirationalist mood in society, manifesting itself through interest in astrology, the occult, etc. (2) A loss of the perception of science as being purely progressive, an emphasis,

not necessarily an overemphasis, on the side effects of technological development such as pollution. (3) Meager financial incentives to scientific careers. (4) A deterioration in scientific literacy. (5) An inevitable loss of infectious enthusiasm that had to come when the exponential, dynamic growth of scientists leveled off in the past decade. These are some rationales, among many. I'm sure you can add some. Yet at just the same time when, in order to assure the future flow of manpower into science, it is agreed that we must work to counter these trends, at the same time there are severe cuts specifically in science education funding. It doesn't make sense. This is the time to put resources into increasing the scientific literacy of the public, in improving science education. We need to train teachers, rejuvenate textbooks, provide creative science programs of the type of "Nova" or "Cosmos" on public television, attract students to research through summer research jobs.

I have been speaking about the universities in terms of their teaching function. Let us not forget that in America, unlike France or the Soviet Union which have alternative science research establishments, it is in the universities that basic research, the foundation of most applied research is accomplished. Let me incidentally assure you that there is no lack of evidence that the basic research of the people before you is being utilized by industry. For instance, the work of mine that was the basis of the Nobel Prize came out in a

recent citation study as the most cited research not in the pure chemistry literature, but in applied chemistry journals.

There is a general synergism between education and research at universities. And when universities in general get in financial trouble, as for instance they are now through the loss of student aid, that is reflected in a general climate of stringency of budget cuts, of constriction on those people most directly concerned with both education and research, namely us. It is not the best of times, especially for the great private universities. And in constricting times it is difficult to do great research and to excite young people.

I mentioned, ruefully, the poor record of industry in supporting research and education in my field. There are signs that things are changing, as industry realizes that its work force comes through us and as it sees its potential supply of employees vanishing. Our Department, for instance, has just received substantial funds for support of graduate training and young faculty from Atlantic Richfield, DuPont, Halocarbon and Exxon. In fact, as I've said, the interests of industry and the universities are not divergent here. I would like to urge you to create, through legislation, some further incentives for industry to support research and education at universities.

Let me turn to another matter, that of curtailment of scientific exchanges with the Soviet Union and its allies. I have some unpopular opinions to express here. Before I tell you my thoughts it's important that you know something of my background. My family and I were under Soviet occupation from 1939-41, in 1944-45, surrounding a worse Nazi occupation 1941-44. I was a child then, but I have learned from my parents how things were. In 1960, in the middle of graduate school, I went to the Soviet Union for a year on a graduate student exchange sponsored by the State Department. I have been in the USSR some three times since. I speak Russian and am probably one of the few people in the U.S. who lectures in the Soviet Union on his work in Russian. I have an excellent idea, I think, of the workings (or lack thereof) of Soviet society, and science and technology in particular. In my research group at Cornell there have worked over the years 3 Rumanians, 2 Czechs, 1 Hungarian, 1 East German and 2 young men from the Peoples Republic of China. My special background gives me a feeling of great sympathy for the people of the Eastern block countries.

In recent years, scientific exchanges between us and the Eastern European countries have been sharply curtailed. The former administration, with the support of Congress, cut exchanges because of the Soviet invasion of Afghanistan, the present one continued this policy because of the tragic Polish situation. Our National Academy of Sciences curtailed official

exchanges and cooperative research because of the treatment of a colleague, Academician Sakharov. There is substantial private and personal pressure on scientists in this country to break all Soviet contacts because of their treatment of refusniks and dissenters. It all adds up to a decimation of visits and exchanges in both directions.

In view of all this let me tell you that I think a serious mistake is being made. I think that despite of actions by the Soviets and their allies, actions that you and I disapprove of, that it is important to maintain contact between scientists of both worlds, and that it is we as much as they who are hurt by the present situation.

In that closed society which is the Soviet Union every small window that is opened on the West brings the light of the world in there, makes friends for us. Soviet scientists, the Soviet intelligentsia, are that segment of their society that is most receptive and responsive to our ideas. They have an opening to the West, a sense of being with us, through the open scientific literature. That sense of being with us is reinforced by a visit by any American there, by any visit here.

There is no question that every such visit, in either direction, has official sanction from their side and is used by Soviet authorities in their own official way. Yet there is no way that they can block the personal side of a visit from coming through— the very presence of an American scientist talking freely about his beautiful experiment, using instruments

they don't have, showing them a picture of his laboratory— that presence by itself makes more friends, convinces more people of what is right here.

I would also argue on simple self-interest grounds that it is essential for the security of our country that we have people here with first-hand knowledge of the workings of the Soviet system. The exchanges and joint research programs produce such knowledge. I would urge you to seek the wisdom of people such as Dr. Armand Hammer, who have shown that it is possible to have meaningful business dealings with the Russians, to our own benefit.

Finally, I think that scientists have a responsibility, based on the rational and open tradition of their activities, to keep talking to each other even when the rest of society is disposed to get angry. It is not that we are better people. Perhaps it's just that we have a base of "small talk, shop talk"— namely the facts and excitements of science— by which an angry discourse is turned into polite friendly conversation. The psychology of communication between people— husband and wife, parent and child, Democrat and Republican, fans of two rival football teams— requires such points of common interest, to allow one to pass over anger. It is no different between nations. If we are to achieve a rational, and secure plan to put an end to the horrible prospect of nuclear war we need to keep in touch. Be firm with the Soviets on the level of governmental relations, but let the scientists talk to each other.

I thank you for being given the privilege and opportunity of testifying before you.

Roald Hoffmann

MEETING THE JAPANESE CHALLENGE IN LARGE SCALE COMPUTING

[By Kenneth G. Wilson]

Executive Summary

The Japanese "Fifth Generation" computer effort will attempt over the next decade to generate computing systems as much as one thousand times more powerful than a Cray-1 in the next decade. This is a very severe challenge to the United States.

There are currently several major barriers to U.S. innovation that could prevent a U.S. response. The barriers are: market requirements of software compatibility, a law of diminishing returns from increased computing power, and the necessity of building software for new systems before a market for these systems becomes evident.

The most serious Japanese threat is that they will accumulate software targeted for their fifth generation computers, especially for industrial computer automation, while U.S. software is restricted to existing systems with far less capability.

The U.S. barriers to innovation can be overcome through a multistage design process for future U.S. large scale systems. A vastly expanded university market would be developed to drive the early stages of computer design. University test and demonstration projects would help separate the design stages; free market competition would be encouraged at each stage.

The university market could be financed by a use tax, namely a sales tax on all computing equipment sold in the U.S., regardless of origin. Other university functions, especially technological training, would also be supported by the tax.

I. INTRODUCTION

A major crisis is developing in the area of large scale computing, namely in mainframes and supercomputers. The opportunity now exists (with the help of new computer architectures) to make a spectacular leap forward in computing power, to roughly 1,000 times the Cray-1 supercomputer. The Japanese are launching one of their celebrated national efforts to take advantage of this opportunity, through their "fifth generation computer effort".

Present market conditions in the U.S. all but preclude a satisfactory U.S. response to the Japanese challenge. In Section II, I will explain the characteristics of the U.S. large scale computing market and show how they hamper innovation. Then (in Section III) the threat posed by the Japanese fifth generation effort will be spelled out, and I will argue in particular that the threat is unlikely to be recognized until it is too late to respond to it.

There is a sensible solution to the crisis. The solution is to vastly expand the university market for large scale computing hardware and software, and to structure this market to provide maximum pressures for innovation, in the context of a multistage computer design process. The solution I present in Section IV is heavily based on my own experience with the university market, and its role in innovation.

To finance the university market, I proposed a use tax, namely a sale tax on computing equipment. See Section V.

The opportunity awaiting the Japanese effort is explained in Section VI. Further technical details are given elsewhere. In Section VII some illustrations of problems demanding very large scale computing are given. Section VIII contains some final comments.

II. BARRIERS TO INNOVATION IN THE U.S.

There are several barriers to innovation by the U.S. in large scale computing. The barriers I will describe are specific to the large scale computing scene and are in addition to general economic problems such as high interest rates.

A. Software compatibility

Most mainframes bought in the U.S. are required to be totally software compatible with existing systems. This requirement is imposed to maintain the viability of existing software, which represents a major investment and is virtually impossible to convert for new computer architectures. IBM has a major research and development effort in Josephson junction technology, in the hope that it will provide a long term growth path for their 360-370 mainframe architecture. But there seems to be no hope that this technology, even when fully developed, can make the 370 architec-

ture competitive with the proposed Japanese systems. The prospects are no more promising for other current mainframes.

Cray Research has carved out a small niche for itself supplying supercomputers to customers willing to sacrifice software compatibility in return for the somewhat greater power of the Cray-1. Cray Research has made enough profit to be able to work on designs for the Cray-2, a supercomputer which is projected to be about ten times more powerful than the Cray-1. However, even the Cray-2 will be far short of the proposed Japanese systems, and there seems to be little hope that further development of the Cray architecture can compete with the Japanese.

There is one U.S. project that potentially could develop into a serious competitor to the Japanese fifth generation computers. This is the Denelocor "HEP" supercomputer, with initial deliveries scheduled for spring of 1982. The difficulties faced by this project are extreme: at the moment not even a Cray-sized market is open to it. Cray Research was able to get started by supplying early production models to the Los Alamos and Livermore Laboratories. Now Los Alamos and Livermore have all but ceased to support innovation; their budget goes almost entirely for the purchase of Cray-1's. This does not mean that they find the Cray-1 totally satisfactory. Not even 50 Cray-1's would provide enough computing power to enable Livermore and Los Alamos to carry out their assigned missions. However, Livermore and Los Alamos now demand either total software compatibility with the Cray-1 or a very substantial immediate advance over the Cray-1. The HEP at present meets neither requirement. Thus the problem for Denelcor is how to win initial acceptance for its product. The HEP is a serious supercomputer effort but its speed is currently not very blinding. Software development for the HEP is only just getting underway. The great strength of the HEP system at present is its large physical memory capacity (up to 1 gigabyte) but the cost of this memory is currently very high.

B. The law of diminishing returns

There is a law of diminishing returns in large scale computing. It states that if a single computing task cannot be carried out on a current mainframe, then a factor of two increase in power is useless for this task, and a factor of one thousand increase in computing power is as likely to be required as a factor of ten. The reason for this law is that tasks that would benefit from a factor of two increase in computing power rarely require more than a minicomputer to carry out—such tasks are extremely unlikely to challenge a mainframe. See Section VII for examples. The typical consequence of the law of diminishing returns is that upgrades from mainframes or large minicomputers need to be by factors of ten, not two, and that over a decade several factor-of-ten upgrades could be necessary. The architectures likely to be explored by the Japanese will allow this level of upgrading: see Section VI. Another consequence of the law of diminishing returns is that an upgrade by a factor of ten does not justify a factor of ten increase in cost.

The law of diminishing returns does not always apply. Consider two examples. A large computing center, presently running twelve hundred jobs a day, needs to expand to accommodate twenty-four hundred jobs per day. The law of diminishing returns does not apply. A factor of two increase in power is fine, and the computing center can easily afford to pay a factor two in cost. The second example is a small research group which has filled up a VAX. Typically this occurs because of individual jobs which are using whole days of computer time. An upgrade to two VAX's is useless at least a factor of ten increase in power is needed and at considerably less increase in cost. This is an example of the law of diminishing returns.

C. The software barrier

Another problem that must be recognized is the software barrier faced by new systems. The need for new, more powerful hardware is often not recognized in advance because no software exists that requires the new hardware. However, the people who prepare software do so only for hardware that currently exists. When a new, sufficiently more powerful hardware system actually becomes available, software for particular applications starts being prepared for the new system, and it is only after this software starts to function that the actual importance of the new hardware becomes evident.

As computers become more powerful, the software needed to take advantage of new computers becomes more complex and takes longer to prepare. Thus there is an increasing delay between the introduction of new computing hardware and the recognition of its importance, as the power of the hardware increases. A super computer can become technologically obsolete even prior to successful software development for it.

D. Limitations of current supercomputers

The Cray-1 is often cited as a model of bold and innovative technology. As a wonder of technology, its kudos are fully deserved. Unfortunately, as a product serving a market both the Cray-1 and the CDC 205 supercomputers are mixed blessings. When judged against the very harsh law of diminishing returns, their computing power is inadequate and their cost is too high for the power they offer. Their physical memory sizes are very limited: both are restricted to a maximum of four million words of memory, which is only four times the maximum memory size of the VAX superminicomputer. Furthermore, the VAX has a virtual memory capability which makes programming the VAX easy for programs using up to 100 million words of memory. The Cray-1 does not offer virtual memory. The "vector architecture" of both the Cray-1 and CDC 205 make them very awkward to program effectively: See Section VI for more details. Finally, while the capability of the supercomputers has remained largely static, the capabilities of the mainframes keeps growing. The recently announced IBM 3081 model K with its virtual memory system rivalling the VAX will further weaken the competitive position of current supercomputers. It is thus not very surprising that there is only a small market for them. The law of diminishing returns also explains why the Livermore and Los Alamos Laboratories, which now rely on Cray-1's, still find their needs unfulfilled. See Section VII.

The difficulties I have cited for the Cray-1 and the CDC–205 are not meant as criticisms of Cray Research or CDC; they simply demonstrate the extraordinarily difficult task now facing U.S. industry in trying to compete with the Japanese effort. Because of the law of diminishing returns they must produce systems expandable to thousands of times more powerful than current mainframes, at not much greater cost than their current offerings. They must provide a software development environment for the new systems that is far more effective than any environment currently available, to make it humanly possible to work with the capabilities the new systems will offer. (The Japanese have included a heavy emphasis on the software development environment in their fifth generation computer plans). There are innumerable ways in which these design efforts could fail. Even if U.S. industry succeeds in reaching the hardware and software design targets, they still face the problem of creating a market for the new systems, since none presently exists for them. The Cray experience shows how difficult these problems will be.

III. THE NATURE OF THE JAPANESE THREAT

A. The danger

The most serious danger of the Japanese fifth generation computer effort is that the importance of this effort may not be recognized in the U.S. until it is too late.

Computer automation of industrial research, development and manufacturing is taking place throughout industry. Over the next ten years a lot of software will be developed in support of this automation, both in the U.S. and Japan. Software developed in the U.S. will mainly be targeted for the most popular computing systems in use in the U.S.: superminicomputers like the VAX, mainframes such as the 370 series, and supercomputers such a the Cray-1. No software will be developed in the U.S. for the Japanese systems until after it becomes clear that they will be sold in the U.S. market and that they will be bought for U.S. research and development efforts. Over this ten year period of software development many occasions will arise where the extra power of the Japanese systems could have been useful; such occasions will quickly be lost because of the futility of pursuing them. Meanwhile, as the Japanese fifth generation effort proceeds, Japanese software designers are likely to start making provisions for the use of their software on the fifth generation systems, with more and more emphasis on software targeted for the fifth generation systems as the details of these systems becomes clarified. The extra power of the Japanese systems will be especially important for computer simulation. See Section VII.

Over a ten year time span, the differences between the U.S. automation software and the Japanese software will grow; at the end of this ten year period visitors to Japan may return with awesome tales of the power and flexibility of Japanese industrial computer automation, just as current Japanese visitors come back with astonishing reports of Japanese industrial robotics.

There is no way that the U.S. economy can survive the competitive advantage the Japanese would gain if they build up a ten year lead over the U.S. in industrial applications of their fifth generation systems. All Japanese industry would eventually benefit from their superior computer automation, not just a few targeted industries like automobiles and integrated circuits.

B. Why the Japanese effort might succeed

Having described the difficulties faced by U.S. industry in trying to compete with the Japanese effort, one may wonder how the Japanese themselves can possibly succeed.

The Japanese have cleverly designed their fifth generation computer effort to avoid many of the difficulties the U.S. industry must deal with. Hundreds of high quality hardware and software engineers (some trained in the U.S.) will be freed from their current responsibilities in order to work on the new project. There is no requirement that they serve an immediate market. They do not have to maintain software compatibility with any current system. They have almost ten years to devote to the project. The full computing design resources of the Japanese computing industry are likely to be available to the project. The specifications for the fifth generation computers (both hardware and software) are bold and vague, leaving the project largely free to follow directions they find most promising. They can take full advantage of economies of scale of integrated circuit manufacture to meet the restrictions of the law of diminishing returns. (See Section VI)

The Japanese are likely to find it more difficult to design and build the hardware and software for their new systems than the U.S. would. What the Japanese actually accomplish in the next ten years could probably be accomplished in five to seven years time in the U.S. The danger is that the U.S. will not even try to compete with the Japanese effort.

IV. THE SOLUTION: A MULTISTAGE DESIGN PROCESS

A. Preliminaries

Fortunately, there is a solution to the current crisis, well suited to the American industrual and university scene and not prohibitively expensive. The solution would alleviate some other important current problems. The solution lies in a multistage computer design process involving close collaboration between industry and universities. The proposal I will outline is based on an example of multistage computer design process which I am personally familiar with. The example is the design history of the Floating Point Systems FPS-164 Attached Processor. Deliveries of the FPS-164 have only just begun, so it will not be surprising if readers have not heard of it or of Floating Point Systems. I will review this history because many details of my proposal are illustrated by it.

B. The FPS-164 design history

The first stage of the design process was a custom built special purpose processor called the AP-120, with no floating point capability, designed and built by Culler Harrison Inc. The second stage was the design of a floating point processor called the AP-120B. Floating Point Systems carried out this design in a very short time in 1975 using a number of the architectural ideas of the AP-120. At the time of the design, Floating Point Systems had only a handful of employees. The AP-120B was sold with no higher level software. It was a spectacular advance in hardware. It was typically bought to be attached to an inexpensive minicomputer; it provided a factor of 100 increase in speed over the minicomputer for roughly the same price as the minicomputer. Programming the AP-120B was difficult, but usually only a few small but time-consuming subroutines were converted for the AP-120B. As a result, the software development problems for the AP-120B were manageable. Floating Point Systems has been growing rapidly ever since as sales of the AP-120B have grown.

The third design stage was the design of the FPS-164 Attached Processor. A commercial verdict on its success is not yet in. However, the main features of the FPS-164 design process are known. The FPS-164 is a further elaboration of the AP-120B architecture. The emphasis was on achieving broader functionality and higher reliability than the AP-120B rather than any architectural advance. The FPS-164 offers greater numerical precision, more memory capacity, and more higher level software than the AP-120B, along with a modern remote diagnostic capability. However, it does not provide as spectacular a jump in capability or cost effectiveness over existing systems as the AP-120B did; for the FPS-164 this jump will be roughly a factor of five to ten over competitively priced systems. The design of the FPS-164 reflects the need of FPS, which is now a medium sized company with income of over $30 million a year, to provide a solid commercial product serving a relatively broad market.

The design of the FPS-164 was preceded by a public demonstration of the capability of the AP-120B archictecture to support a broad range of scientific applications. This demonstration was carried out at Cornell University. The Cornell Computing

Center, in collaboration with a consortium of scientific researchers with large-scale computing needs and very little money, bought an AP-190L array processor from FPS in 1977. It was attached to the central university computing system: an IBM 370-168 computer. The computing center sponsored the development of a primitive Fortran compiler for the AP-190L, which enabled consortium members to use the AP-190L for a variety of scientific applications. The software developed at Cornell was considerably more complicated than the software developed in minicomputer applications. The Cornell compiler fell far short of a fully mature higher level software support system and the consortium users had some painful software development headaches.

The Cornell example provided Floating Point Systems with a wealth of experiences of the advantages and weaknesses of the AP architecture for general scientific use. The Cornell example provides potential FPS-164 customers with a model for its use; the Cornell experience is likely to play in important role in marketing the 164. Cornell will be an early demonstration site for the FPS-164.

C. General description of the multistage design process

For the U.S. "fifth generation" effort I propose a framework generalized from the experiences of the FPS-164 development.

The U.S. effort should employ market forces to push a multiple stage design process, with heavy competition and rapid innovation at every stage. Since no present market encourages innovation in large scale computing a market must be created for this purpose. I believe the only workable approach is to support a vast build up of the university market for large scale computing systems. Each design stage would be a separate, continuing process, because of the continuing nature of innovation in computing technology. The first stage would involve basic architectural innovations in hardware with very few facilities and no software. The Denelcor HEP will be in this first stage until enough software is developed for it. The first stage systems would be sold in small numbers for very small user communities, possibly some computer scientists interested in experimenting with the new architecture, along with users with very specialized applications that are feasible despite the limited capabilities of the system. These users would accept the burden of living with the instabilities and very primitive support facilities provided in return for unlimited access to the system. These experimental, university-based operations would double as initial public demonstrations of the capabilities of first-stage systems, in analogy with Cornell's experience with the AP-190L.

Subsequent design stages would take the most effective architectures and refine them, adding more hardware facilities, building up higher level software, in general exploiting the basic architectural framework for successively larger and broader markets. Each design stage would be short, with limited objectives and under intense pressure to complete the design and bring out a product to market. Each new design stage would be an opportunity to incorporate successively more modern integrated circuit technology into the design, to make use of the latest diagnostic and quality control procedures, etc., besides incorporating the lessons of the experiences of previous stages with the same architecture. Universities would typically buy the first few production models of intermediate stage designs, for test and demonstration purposes, but much of the follow-on market for these systems would be commercial, for special purpose applications. The final stage of the design process would involve industry giants, like IBM, preparing systems for worldwide distribution with very general capabilities, proven software and full maintenance support. The last design stage would be concerned heavily with maximizing software compatibility with the manufacturer's previous product lines, as well as many detailed optimizations involved in serving a multi-billion dollar market.

The uses of the university demonstration system should be as varied as possible to maximize the effectiveness of the demonstration. All kinds of basic research should be carried out. Theoretical physicists, chemists, astrophysicists and applied mathematicians would carry out simulations of real or fanciful physical situations. High energy physicists, space scientists and others would analyze enormous data streams from their experiments. Innumerable engineering, data base, and information retrieval applications should be pursued. Musicians, artists, humanists and social scientists might find uses to try. The relevance of any of this research would be unimportant. The only requirement would be breadth—a broad range of research activities would be needed to provide a broad range of tests for the new computing systems. It is important, however, that the computing systems being tested be general purpose systems, rather than specific to the research being pursued.

Many systems would fail at various stages of the design effort. The market supporting innovation must be large enough to survive these failures, and encourage

only the most successful technologies through further design stages. The university projects which would be the primary market for products of early design stages would also have a high failure rate. The demands for innovation from the university demonstration projects would be severe, and only a small fraction of the university projects would be fully successful. The university efforts require discovering strengths and weaknesses of a new system despite very considerable barriers of instability and inadequate software, finding novel applications for the system which would broaden its commercial market, and developing the initial framework for how the system could be used. These are not easy tasks. The Cornell Array Processor project was a success, but in many ways we were lucky to succeed and I could not guarantee success for any specific university project which tried to be as bold as we were.

NASA has started a design competition for a special purpose large scale computing system, which they call a " numerical wind tunnel". As far as I can tell, this design effort has not passed the first stage of architectural tests. yet. NASA should be encouraged to commission a short term first stage design effort for their numerical wind tunnel project, to test the general architectural framework for their system. The result should be subjected to the same university test and demonstration effort that the Denelcor HEP should receive.

D. Benefits of the multistage plan

There are many attractions to the model of a multistage design process with heavy involvement by universities. University research groups are well-suited to work with new computing equipment. An abundance of graduate and undergraduate student labor eases the difficulties and frustrations of working with new equipment. Faculty members carrying out research on these systems often have a high level of expertise which enables them to over come difficulties of poor software and inadequate documentation. The competitive character of university research causes both faculty and students to seek out new or unexpected applications of the system in order to obtain a research advantage.

The training function of university test and demonstration projects cannot be overemphasized. In an era of rapidly developing computer technology, technology which is already established when a student enters a university is on its way to obsolescence by the time the student graduates and enters the work force. Providing students with experience in computing technology in advance of commercialization is the only way for them to be reasonably current at graduation. New systems, by virtue of their instability and poor software support, can be the source of meaningful part-time jobs for many undergraduates. Furthermore, good students are attracted, rather than repelled, by innovation and can show incredible patience in front of a computer terminal.

One of the very important reasons in favor of the multistage design approach is that software development and experience can begin long before all design stages are complete. It is important that software experimentation for new systems be as bold as possible, because of the truly awful state of current software development frameworks. User research groups trying out new hardware should be strongly encouraged to work closely with computer science groups or commercial software efforts, to try out all kinds of new approaches to software development. It is important that individual research users be presented with thousands of hours of computing time on systems of awesome power and untried architecture. This will bring users face to face with the software development problem in its most extreme form, and thereby provide maximum motivation for users to help find a solution to it. To maximize the ease of movement of experimental software in universities, powerful, stable UNIX and LISP-based systems should be made widely available in universities for software development work. (UNIX is a trademark of Western Electric, Inc.) Plenty of support should be available to research groups to buy software and to hire students and support staff to help flesh out experimental software systems in order to meet their practical needs.

A university test-and-demonstration project for a new commercial product is a superb framework for technology transfer from universities to industry. University researchers working with a new piece of equipment are strongly motivated to find ways to enhance the equipment to make it more effective for their research. Especially for computer equipment, enhancements are likely to be helpful to a broad class of users of the equipment, because of the inherent flexibility of computers. A small company or a new product group which is trying to win acceptance for a new product has a strong motivation to listen to the ideas of the university users, especially if the success of the university demonstration project is crucial to the future of the product. Interaction between university researchers and an industrial design

group is much easier and more effective at a very early stage in the design process, when major changes can be made easily and university personnel are working on a personal basis with the design group. Frequent interchanges take place as the researchers encounter difficulties and seek the help of the designers in dealing with these difficulties.

The improvements developed in the course of a university test operation may seem minor in the early phases of a new product. However, if the product develops to the point of serving a multi-billion dollar market, the economic impact of the university's efforts can be profound. In contrast, once a product is in the final stages of preparation for a billion dollar market, a massive bureaucracy is in place surrounding product design, development, and marketing and any effective influence by a few university researchers is hopeless.

The university test and demonstration projects would develop strong ties with potential commercial customers for new systems. Cornell has fielded many inquiries from potential commercial customers for the AP-190L or the FPS-164. We accept site visits, and play a strong role in the Floating Point Systems user group. At user group meetings, university and commercial users of Floating Point Systems get together to exchange experiences and software, in the process establishing another university-industry link where technology transfer can take place.

V. FINANCING THE UNIVERSITY MARKET

The critical requirement to proceed with the multistage design effort I propose is adequate financing of the university market. For the university market to push innovation in large scale computing adequately, hundreds of university research groups must be able to purchase multi-million dollar computing systems in early stages of development. There must be adequate support for ancillary equipment, for junior faculty, graduate students, undergraduates, and computer support staff who would all work with these systems, undergraduates, and computer support staff who would all work with these systems, and for software purchases. The present means of university financing are hopelessly inadequate for this purpose.

It is clear that the technological functions of universities in the modern U.S. economy are all tied to computing technology in one way or another. The very great concern about trained manpower is almost entirely due to the computer; computing technology enhances the value of highly trained personnel and generates a greatly increased demand for them. Every time a new computer is bought, a new source of demand for trained manpower and new technology is created—new demands for programmers, maintenance personnel, ideas for computer applications, along with a demand for computer technology development to produce future upgrade capabilities for the computer itself. All these demands place new strains on the U.S. university system. To keep up with these demands, as well as support the university market, it is imperative that support for universities keep pace with the growth of computer technology itself.

The financing needs of universities could be met, fairly easily, by means of a use tax, namely a retail sales tax on all computing equipment sold in the United States, regardless of origin. This use tax would distribute the costs of university support on a roughly equitable basis among the chief economic beneficiaries of the universities' functions. The use tax would define a natural and reasonable scale of university support, determined by their principal economic functions. It would enable university support to grow faster than inflation as long as the computing industry itself is growing faster than inflation; this is a critical requirement at the present time. It would provide new incentives to the universities themselves to maximize their effectiveness in serving the high technology economy.

No one likes a new tax. However, in the case of the proposed computer tax I think it would be exceptionally difficult to prove that anyone would be harmed even by a very stiff tax. The computer manufacturers would all be treated equably by the tax with respect to both domestic and foreign competition, and would benefit enormously by the increase in trained personnel that would swell their potential customer base. Large commercial users of computing would benefit from more rapid innovation in computing technology, and an increased trained manpower pool. In addition, the costs of computing equipment are so volatile that the impact of a computing tax phased in over several years would be difficult to identify.

There are many details of implementation of a computer tax that would have to be worked out. Would it be passed on to universities through granting agencies like the National Science Foundation, or by other means? Would industry be encouraged to support universities directly, through deductions from the tax? How would "com-

puting equipment" be defined? I have no special expertise to offer for these questions so I will not discuss them.

VI. PRESENT AND FUTURE COMPUTER ARCHITECTURES

In this Section I will try to explain without technicalities the opportunity that now exists in large scale computing. The opportunity exits because of the rapidly shrinking size and costs of very large scale integrated circuits; it is now possible to build very powerful processors from just a few integrated circuit chips.

The route to extraordinary advances in computing power is through massively parallel systems—systems of many thousands of processors in a closely coupled system. Technical details of how such a system would function are discussed in a separate paper; here I want to review the most essential characteristics of different architectures for handling parallel processing.

There are three ways to organize parallel systems, illustrated by the Cray-1, the Illiac IV, and the Denelcor HEP computer respectively. To explain the differences, I will use a human analogy. Consider an airline counter, with a number of clerks available to serve a line of customers. A standard computer responds to a single clerk serving a single queue of customers. IBM's Josephson Junction effort is analagous to trying to speed up processing by a single clerk. The Cray-1 corresponds to an assembly line arrangement. There is still a single queue of customers and they all start with the first clerk, but chores like ticket writing are handled by the assembly line: the first clerk writes in the customer's name, the second clerk writes in the destination, etc.

In the Illiac IV arrangement, the head clerk has a megaphone; the remaining clerks each have separate queues of customers. The head clerk barks out instructions which all the remaining clerks must follow. When the head clerk calls out "write the customer's name on the ticket", all the remaining clerks simultaneously write in their customer's name on their ticket. If one customer wants a special service, like tracing lost baggage, the remaining clerks stay idle while the head clerk barks out a sequence of instructions for baggage tracing.

The Denelcor arrangement is the arrangement actually used for airline counters. A number of clerks stand behind each counter, each able to carry out any transaction requested by the customer, regardless of what the other clerks are doing. The customers line up in a single queue. The customer at the head of the queue goes to the first available clerk.

Now suppose there are ten thousand clerks behind the counter. A single queue with one person at a time going to the counters could be a bottleneck. The "Ultracomputer" project of New York University has a variant on the Denelcor arrangement that alleviates the problem. In the ultracomputer arrangement, when a clerk has finished processing a customer, he goes to the queue to fetch a new customer. If two clerks bump into each other on the way to the queue, one clerk stands aside; the other clerk claims two customers from the queue, brings them back, and turns one over to the clerk who stayed behind. If there is a second collision between two clerks, both expecting to fetch two customers, then one stands aside and the other clerk fetches four customers, releasing two to the clerk who stood aside. As a result, large numbers of customers are pulled off the queue at a time.

In the IBM, Cray-1, and Illiac IV analogy, the queue of customers is analagous to the stream of elementary machine instructions (add, or multiply, or branch, etc.) which come from a user's program. One of the drawbacks of the Cray-1 and Illiac IV architecture is that ther programmer has to be thinking about the impact of the Cray or Illiac IV architecture on each and every line of code he writes. In the Denelcor or Ultracomputer examples, the queue of customers can correspond to a queue of entire loop bodies, or whole subroutines to be executed or even a queue of entire jobs; there are correspondingly less architectural constraints per line of code. However, the most important advantage of the Denelcor-Ultracomputer approach is that the 10,000 small, inexpensive chip set processors. Chip sets can now be extremely powerful. For example, Hewlett-Packard has just introduced a 64-bit multiplier chip with a one microsecond cycle time. 10,000 of these chips would have a maximum through-put rate 10 times the projected top speed of the Cray-2, or 100 times the Cray-1. The present Denelcor processors are large and expensive, not small and cheap, and the present plan is to allow a maximum of 16 processors per system. However, if the Denelcor architecture proves successful, further design stages can be used to design inexpensive components for the Denelcor architecture. Modifications of the ultracomputer type can then be used to raise the maximum number of processors from 16 to many thousands.

The Cray and Illiac architectures are very difficult to extend to the same level of computing power that a Denelcor machine with ten thousand processors would have.

The law of diminishing returns requires that the proposed ten thousand processor systems have a reasonable cost. This should be possible, through the enormous economies of scale involved in silicon ship manufacture. As long as the new systems have only a few chips per processor, the cost should not be all that high for thousand processor systems. Furthermore, the Denelcor arrangement is upgradeable, presently from 1 to 16 processors. Future ultracomputer architectures would be upgradeable from 1 to 10,000 processors.

VII. EXAMPLES OF LARGE SCALE COMPUTING PROBLEMS

In this section, I will describe very roughly some of the applications that demand computing power beyond the Cray-1 and illustrate the law of diminishing returns. The applications are of broad practical and industrial interest: I will try to correct the widespread misconception that only Los Alamos, Livermore and the Weather Bureau need Cray-level computing.

Consider the problem of simulating an integrated circuit chip. I will greatly simplify the problem. Computer designers like to talk about "gates", and so shall I: a gate will mean a very small object that is either open or closed. An integrated circuit contains a number of gates. Every clock cycle each gate opens or closes according to the rules of the circuit. A computer simulation stimulates a large number of clock cycles; for each cycle the computer must check each gate, using the circuit rules to determine whether it is open or closed. As the number of gates on a chip increases, the number and complexity of the chip's functions increases also. This means the number of cycles needing simulation goes up. For simplicity we shall suppose that the number of cycles simulated is proportional to the number of gates on the chip.

An artificially simple chip would be a one-dimensional line of gates—say from one hundred to one thousand gates total. The simulation of a chip with one hundred gates for one hundred cycles requires ten thousand gate checks. A personal computer could handle this simulation. An increase to two hundred gates would require a concomitant increase to two hundred cycles of simulation, thus giving of four-fold increase in simulation time; a personal computer could still handle the problem. Even an increase to one thousand gates could be handled easily on a small minicomputer. These tasks do not challenge a mainframe.

A realistic integrated circuit chip is two-dimensional. Instead of one hundred gates, it can have one hundred X one hundred gates and up; a thousand by a thousand gates are being talked about. The switch to two dimensions increases the simulation time by at least a factor of ten thousand (a factor of one hundred for the extra gates times a factor of one hundred in the number of cycles)! Now the simulation takes a fair amount of time on a mainframe. Furthermore, a reasonable increase in complexity is to double the number of gates in each dimension. Then the total number of gates is quadrupled; the number of cycles is also quadrupled. The total simulation time goes up by a factor of sixteen. Thus each time the number of gates along each dimension is doubled, a factor of sixteen upgrade in computing power is needed. This is an example of the law of diminishing returns. Simulation is cheaper and faster than actual prototyping of expensive or complex systems, and allows for greater exploration of design alternatives.

Another potent area of large scale computing is in the simulation of dynamic processes. Simulation will be an increasingly important aspect in industrial research and development. Much simulation work presently concerns matter in lumps: for example spacecraft trajectory simulations where the whole spacecraft is treated as a point-like object. But matter does not always behave as a lump. Metals flex and vibrate, liquid and gas flows become turbulent. To simulate processes involving deformation, the dynamics must be described in terms of the behavior of a large number of small volume elements of the matter in question. Normally one would expect to need hundreds of volume elements in each dimension, resulting in millions of volume elements in total. For a three dimensional object. Realistic simulations of three dimensional systems are usually beyond the capability of even the Cray-1, but would start becoming feasible on the Japanese fifth generation systems. The simulations performed at Livermore and Los Alamos on their Crays are mostly simulations of simplified two-dimensional models. Multi-dimensional simulations suffer from the same law of diminishing returns as for the circuit chip simulations. Both the circuit chip and deformable dynamics simulations are easily adapted to the Denelcor architecture because of the large amount of computing required per time

step; the simulation of many gates or many volume elements in one step is easily divided among may processors.

There are other areas of demand for very large scale computing, if supplied at reasonable cost. Current research on large data base systems, for example, at the University of Wisconsin, is concentrating on parallel processing to enable searching of enormous amounts of data in a reasonable time. Very high level programming languages are being designed, such as the SETL language from New York University, which should enable programmers to tackle much more complicated tasks than current languages like FORTRAN or ADA. Unfortunately, programs in languages like SETL run very slowly on current computing systems.

It is likely that the most important applications of the Japanese systems will be in areas I have not identified at all, that being the history of the most successful computing systems of the past, from pocket calculators on up.

VIII. CONCLUSIONS

I have tried to give a practical view of the challenge to the United States of the Japanese fifth generation effort. It threatens to give Japan a competitive edge in a wide range of industries through improved computer automation. It threatens to give the Japanese computer industry an integrated computer product line of spectacular power, cost-effectiveness, and upgrade capability. I have outlined a multistage design process with heavy university involvement as early customers of innovative large scale computing systems. I have argued that this plan fits in very well with the overwhelming need to increase time number of students with serious training and experience with computing. I have proposed a plan to pay for the university involvement.

However, the Japanese challenge goes beyond immediate practical considerations. The Japanese threat is to take away one of the last U.S. frontiers—the frontier of computing technology. Already, top quality university computer science researchers are troubled by the prospect that their research will be picked up by the Japanese, to be used againt us, while the same research is ignored by U.S. industry. The future will bring even more serious dilemmas, as the best U.S. industrial computer designers (both hardware and software) face the choice of signing up with the Japanese project, in order to work at the frontier of computing technology, or stay with U.S. firms and work on less challenging, less adventurous tasks.

It is essential that the present U.S. lead over Japan, due to companies like Denelcor, Inc., not be dissipated. Industry, universities and government must unite behind a plan bold enough and sufficiently well financed to keep the U S. at the forefront of large scale computing technology.

The potato growers have arranged, through a friendly Congress, for a mandatory fee on all potato growers to support a "potato board" in the Department of Agriculture. The potato board supports research on diseases of the potato, and other activities deemed important by the potato industry. It is time for the high technology sector of the economy to arrange some similar framework, albeit on a very much larger scale, to restore the essential high technology functions of the U.S. university system, and to restore U.S. innovation in large scale computing.

Chairman FUQUA. Doctor Wiesel?

[The biographical sketch of Dr. Wiesel follows:]

DR. TORSTEN NILS WIESEL

Born in Upsala, Sweden, June 3, 1924.

M.D.—Karolinska Institute of Sweden, 1954.

Instructor in Physiology, Royal Caroline Medicosurgical Institute, 1954–55.

Fellow in Ophthamology, School of Medicine, Johns Hopkins University, 1955–58.

Assistant Professor of Ophthalmic Physiology, Johns Hopkins University, 1958–59.

Harvard Medical School: Associate in Neurophysiology and Neuropharmacoloy, 1959–60; Assistant Professor, 1960–64; Professor of Neurophysiology, 1964–67; Professor of Physiology, 1967–68; Professor of Neurobiology, 1968–74; Robert Winthrop Professor of Neurobiology, 1973 to date; and Chairman, Department of Psychiatry, 1973 to date.

Jules Stein Award, Trustees of the Society for the Prevention of Blindness, 1971.

Rosenstiel Award, 1972.

Member: AAAS, American Physiology Society, American Academy of the Arts and Sciences, Swedish Physiology Society.

Research Interest: Neurophysiology, especially the visual system.

Nobel Prizewinner in Medicine, 1981.

STATEMENT OF DR. TORSTEN N. WIESEL, WINTHROP PROFESSOR OF NEUROBIOLOGY, HARVARD MEDICAL SCHOOL, BOSTON, MASS.

Dr. WIESEL. Yes, Mr. Chairman and members of the committee. I will probably bring us back home to the reality in which we live here and I particularly ask myself as a biologist and medical scientist, what are those problems that we face?

Looking at the situation right now and what has been built up during the last 30 years, I think we all feel that the basic science establishment that has been built up is a treasure—it is a national treasure in a way—and that we have to protect and see that it can continue to develop and grow. I think if you stifle this growth, it is like a plant, you will prevent it from developing naturally. Science has to grow in a way that can not be directed by government, it has to have its own forces. It has to be supported generously enough so that it can have that growth.

One also likes to remove this feeling that the scientists sort of take the money and do not return anything. If you study the results in terms of investment of money into science, it is a very good investment. I would like to refer to a study that was carried out by NIH. They looked at 10 specific research projects which have developed new methods, of various kinds, and it was found that these projects today or last year, added $37 billion to the gross national product because of these techniques developed in these laboratories and now being used by various industries.

So, I think it is very important to emphasize that every dollar invested in basic research is going to pay off very good dividends. It is not the depletion of the national resource but actually an addition to it.

Even if universities are primarily looked upon as educational institutions—and that is, obviously, an important function of all of the great universities—they are also being very productive in terms of developing not only skills for individuals but also new technologies and new opportunities for industries to develop them.

In biology there have been in recent years—in molecular biology—developments of new industries, recombinant DNA, for example. I think the new areas of hybridoma techniques where one can produce monoclonal antibodies, are going to be tremendous areas of development with obvious industrial implications in the future. In addition, of course, it will be possible to have much more adequate treatment of a number of diseases.

In biology, and I suppose in all areas of science, the budgetary increases have been leveling off in recent years and because of inflationary pressures our budgets have, in fact, been reduced. Yet, somehow we have been able to cope with that in the last 2 years in terms of purchasing value. But, we have somehow come to the point now when we are at the wall and this is a crisis situation so far as I can judge.

I think it is a crisis, for example, when NIH calculates or estimates that they will only be able to pay 15 percent of all research application approved by the peers. That means that only 15 grants out of 100 which are approved by the peers to be good research application can be funded. It is estimated that in 1979 at NIH there

were 5,600 grants, approved by peers but not funded; by 1983 it is estimated it will be over 11,000. So, why is it, of course, that the funds have suddenly seemed to have halved, even if inflation causes some pressures. I think one reason is that it is much more expensive to do research today. The cost of equipment has increased much more than the cost of living index for example. Why that is, I do not really know, but it is a fact.

Second, to do biological research you have to use sophisticated equipment and that is expensive. So, I think on average at the NIH in the last few years, the individual costs, or individual grants, have doubled practically. We obviously do not have a corresponding increase in our budgets during this period.

One serious aspect of these restrictions is obviously, as has been pointed out before, that we will not be able to attract young people to train in these areas. I mean who, in a sane mind, would spend 15 years of education and training knowing there is only a 15 percent chance to carry out what you have been studying for through these very long and arduous years. We ought to see that we train and educate people but also provide a reasonable future security for people who go into science. Again, I would like to bring up the fact that these are productive people in the society. They are not draining but adding to the skills.

A few years ago, in 1964, the United States put in 2.94 percent of its gross national product into research and development. In 1981 it was 2.27 percent, a decrease of more than half a percent. During the same period, Japan has increased their figure from 1.48 percent to 1.93 percent, nearly 2 percent. They are still below us but I am sure that if the present trend continues they will exceed us.

In Germany there is a higher investment in research and development than the United States at the present time, and they were way under us in 1964. So, we are slipping. There is no question that we are slipping. One just has to travel abroad and go into a laboratory in Switzerland or Germany or in Japan and see what kind of equipment they have, and the resources made available to them. We just do not have those resources.

Another reason you have to appreciate is that there has to be growth in science because we train people and they in turn must be able to use their skills. The trimming of the growth, the pruning of the growth occurs through the peer review system, because as new branches develop, other branches will not be able to compete and are trimmed. This is a natural growth process, but we have to see that money is available to let the tree grow in a normal fashion and that is not the case at the present time I believe. This is particularly hard on young investigators because they are not in the same position to compete as are established ones. Many of the panel members here have come to this country because there are excellent opportunities to do original and independent research. To me, that is one of the great features of this country—that you are free to do research, that you get the money and you can do your own research—whereas in Europe there is much more hierarchy—and some big boss tells you what to do.

This country's scientific and technological strength has developed because it is a model system of scientific support that we have. I

know of no other country who does it better. That is why it is such a tragedy to train our people and then they cannot find money.

In the last Science magazine, there is an article by Allan Bromley in which he points out that there is an educational problem, a serious situation in providing training for young people who will go into science. He then points out that only 105,000 high school students in this country take calculus, whereas 5 million Russians do. He points out that Japan produces 5 times more engineers than the United States, and so on.

Again, to repeat what Dr. Bloembergen said, we look fine right now because we have all these Nobel prizes. But this refers back to work done 20 years ago. When the year 2000 comes we will not be here. We will not, perhaps, have the opportunity to invite any American Nobel prize winners at all. So, I can just urge the committee, and I know that you are very sympathetic to the general point of view that I have espoused, to bring forward these points: that science is a good investment and, in addition to all the other things that new knowledge does, and new ideas do, it can be defended on that basis. Thank you.

Chairman FUQUA. Thank you very much, Dr. Wiesel. Your written statement will be inserted into the record.

[The prepared statement of Dr. Wiesel follows:]

Congressional Testimony

Torsten N. Weisel

Harvard Medical School

February 25, 1982

Mr. Chairman:

It is a great honor for me to be able to testify today before your committee on Science and Technology; I appreciate this opportunity to present my views on research.

In the past three decades the United States has, through the generous support provided by the federal government, become preeminent in biological and biomedical research. This support has been rewarded by spectacular progress in these fields, so that we have gained considerable new knowledge in the mechanism and treatment of disease. The advantages of this research have not been limited to the physical and mental health of the populace, but have also provided considerable benefit to industry. Innumerable technological advances have resulted from biomedical research ranging from fiber optics and computer technology to recombinant DNA and hybridoma techniques. The economic benefits of most of these developments have only begun to be realized. One study showed that ten selected NIH supported projects have resulted in a contribution of $37 billion annually to the GNP, which is something like a hundredfold return on the initial investment. It has been suggested that private industry might somehow fill the void left by cutbacks in federal support of research. This is very unlikely, first because the ultimate benefits of basic research are often unsuspected when the research is begun, and also because it is difficult to restrict the results of the research to the company that funds it. A clear

example of this comes from the field of molecular biology. Recombinant DNA research has over the past two or three years become a very popular area for private investment. That technology depends, however, on decades of fundamental research that was supported principally by governmental funds. Industry favors those expenditures which can be counted on to make a profit. Only the federal government can assume the risks and generate the funding necessary for a basic research enterprise of the magnitude required to meet the Nation's technological needs.

In view of what I have just said, it is particularly disturbing that the predominance that the United States has enjoyed over the last few decades is seriously threatened. In a period of budgetary restriction, research requires increasingly sophisticated instrumentation, so that the budgetary needs of staying at the forefront of research are accelerating well ahead of inflation. Other countries, including Germany, Switzerland and Japan, show much more willingness to adequately equip their laboratories for basic research. For years the best scientific minds came to the United States because of the opportunities to carry out independent and original research. Now I have noted a number of disturbing instances in which prominent American scientists are moving elsewhere to pursue their research under conditions that are better than those available to them here.

Another aspect of research that is also threatened is the ability to train enough personnel to staff our laboratories in the future. In a way this is the most serious problem with budgetary cutbacks because its effect is felt for decades. It takes ten years to train a competent scientist

after his or her graduation from college. Because the colleges and high schools are also affected by these restrictions the effects of the present policy will be manifested well into the next century. Bright young men and women must be identified, encouraged, trained, and supported in a constant stream if the research enterprise is to continue to flourish. The competition for outstanding young people is keen, but for those who choose science, the training programs are long and arduous, and the prospects for success are highly uncertain. Funding for training and research is becoming more difficult to obtain. Figures from the National Institutes of Health reveal that the number of approved but unfunded research grant applications rose from about 5,600 in Fiscal Year 1979 to an anticipated 11,000 in Fiscal year 1983. It is virtually impossible to overstate the longterm consequences of such figures in terms of the impact insufficient funding has upon the education and training of young scientists. In the past ten years the funding for research and training has been irregular and unreliable. To attract talented young people to science it is necessary to provide much more stable support, so that science can be viewed as a career, with reasonable security and future prospects. It is easy to become complacent about the capabilities of our scientific establishment when year after year Americans win so many scientific prizes. but the work upon which these prizes are based occurred 20 years previously, and the scientists who recieved these awards began their training 30 to 40 years ago. Our lack of support for science today was brought out recently in an article by Allan Bromley in Science magazine. he points out that only 105,000 of all U.S. high school students take any calculus at all, while 5 million in the soviet Union take 2 years of it. Japan graduates 5 times as

many engineers as the U.S. Clearly our position in science and technology in the next 40 years is threatened.

At a time when our country's worldwide preeminence has been lost in so many fields and is being challenged more vigorously than ever before in others, we still retain the leadership in biomedical research. It would be foolhardy in the extreme to risk forfeiting that leadership. The necessary investment can certainly be justified on humanitarian grounds, but the benefits to the national economy lend equally strong support to the argument for adequate federal funding of basic research.

Thank you, Mr. Chairman, for inviting me to come here today.

Chairman FUQUA. Dr. Hubel?

[The biographical sketch of Dr. Hubel follows:]

DAVID HUNTER HUBEL

Born in Windsor, Ontario, February 27, 1926. U.S. Citizen.
B. Sc.—McGill University, 1947.
M.D.—McGill University, 1951.
Honorary Degree: D.S.C. McGill University, 1978.
Fellow in Neurology and Neurophysiology, Montreal Neurological Institute, 1952–54.
Resident Neurologist, John Hopkins Hospital, 1954.
Neurophysiologist, Walter Reed Army Institute for Research, 1955–58.
Senior Fellow in Neurophysiology, School of Medicine, Johns Hopkins University, 1958–59.
Assistant, Associate, and Full Professor of Neurophysiology, Harvard Medical School, 1958–67. Chairman, Department of Physiology, 1967–68. George B. Berr Professor of Neurobiology, 1968 to date.
Senior Fellow, Harvard Society of Fellows, 1971 to date.
James Arthur Lecturer, American Museum of Natural History, 1972.
Ferrier Lecturer, Royal Society of London, 1972.
Harvey Lecturer, Rockefeller University, 1976.
Grass Foundation Lecturer, Society of Neuroscientists, 1976.
Award for Outstanding Ophthalmic Achievement, Research to Prevent Blindness Trustees, 1971.
Lewis S. Rosenstiel Award, Brandeis University, 1972.
Friedenwald Award, Association of Researchers in Vision and Ophthalmology, 1975.
Karl Spencer Lashley Prize, American Philosophy Society, 1977.
Member: National Academy of Sciences, Leopoldina Academy, American Academy of Arts and Sciences, American Physiology Society.
Research Interests: Neurophysiology and neuroanatomy of the visual system of higher mammals.
Nobel Prizewinner in Medicine, 1981.

STATEMENT OF DR. DAVID H. HUBEL, BERRY PROFESSOR OF NEUROBIOLOGY, HARVARD MEDICAL SCHOOL, BOSTON, MASS.

Dr. HUBEL. Mr. Chairman and members of the committee, one of the advantages of being last on the list is that everything impor-

tant and conceivable almost has been said. It certainly is a challenge to try and think up something new and different.

A high proportion of the science winners this year in Stockholm were, obviously, American. It was interesting for me to sit back and try and think why this is. I was able to come up with three good reasons and there may be others.

One is obviously the generous and imaginative use of money that our Government has made in supporting science over the years and this continues despite the fact that, relative to the sixties, we are in a period of some famine. Things still are not that bad.

One great feature of the system of funding that we have in this country that I would like to emphasize and which has been mentioned by several others here, is the system of peer review. Occasionally one hears this threatened as being expensive or cumbersome, and perhaps it is. But, believe me, it is absolutely a marvellous system if one compares it with the kinds of systems that our friends in Japan and Western Europe have to contend with.

The knowledge, when you sit on a study section and judge other people's grants, that those same other people will be sitting on other study sections judging yours, insures fairness. An immediate knowledge of the fairness that prevails at these committees is certainly something very impressive. So, I would like to put in a very special plea for the continuation of that.

The other thing I would like to emphasize is the need that we have for consistency. This is a nation of wild enthusiasms. One only has to remember the hoola hoop and the bubble bath and other crazes that sweep the country. One craze was this sputnik craze, which did a lot for science but had the unfortunate effect of force-feeding scientists who were not as good as they ought to have been, and those people are the ones that are suffering now, when the screws get put on. So, not only should we avoid famine where possible, but we should dampen down our enthusiasm in good times and not expect that overnight we will be able to train huge numbers of scientists who previously did not exist. A consistent, slow, rate of growth is probably the thing we need more than anything else, certainly more than these wild swings that we are subject to.

The second reason, I think, why so many of us are sitting here at this table is the excellence of our universities. Here I am thinking not particularly of the teaching of students but as places to do research.

If you look at what prevails in Western Europe and Japan, one has to say that the universities, in that respect, are utter disasters. We are very lucky indeed to live in a system where a young person can not only get money from outside sources to do his research, but is not dominated by restrictive and self-interested chairmen. The independence that a young scientist has in our universities is unmatched anywhere else. For this reason, one looks with great concern over the plights that our private universities seem to be in and seem to be facing in the future in this country. So, if anything can be done to help universities, those things should be done. They cannot, apparently, continue to exist on the fees they charge their students. As a Harvard professor who can hardly afford to put three boys through college, I can say that the fees cannot be raised any more than they are, so some solution has to be found.

The third thing that I think we should not lose sight of is that, of the people sitting at this table, four of us are from other countries and have received a substantial part of our training in other countries. We should not forget the debt that we owe, to Western Europe particularly, for a lot of the science that we have. We would just be blindfolding ourselves if we fail to recognize the importance of this. I say this because there seems to be an increasing threat to international exchange, not just with the Soviet bloc, but in the possibilities that one has of bringing in people from West Europe and Japan for training and sending people abroad for training. These things are looked upon by our Government and probably by our society, I think, with a great deal of suspicion. As though every time one went to an international meeting abroad, one was doing it for the sake of having a nice trip and partaking in some sort of junket. But, believe me, these exchanges are terribly important for scientists and for everybody else for many different reasons.

After the week in Stockholm I was able to go to Finland and to Austria because of an imaginative policy on the part of our Government to bring Nobel Prize winners to these two countries. It was in Finland, of all places, looking around labs there that I discovered that they are better equipped in many phases of their armamentarium than I am at Harvard and that is a bit sobering. One expects that perhaps in West Germany or Japan but not particularly in what is normally regarded as an underdeveloped country. When I find myself making lists of the models of computers that they have and their interference filters, well that is—incidentally both of those from the United States—that is a bit sobering. But, also you cannot turn on a television set in Western Europe or Japan without realizing that their television is not only better in its programing, but it is technically better, of all things. They have more dots per square inch and the colors of the faces are not green or orange—in hotels, mind you, not just department stores. You turn on a television set in Japan at 8 p.m., some prime time, and you have a lecture on analytical geometry—you have other things to choose from, of course, but every program is not a situation comedy. They are doing better than we are. Their carpenters do not have to add seven-sixteenths to three-eighths in their calculations, they have been on the metric system, along with the rest of the world, for a long, long time. We seem to be doing very little toward even that simple-minded end. Getting abroad and seeing other countries not only tells you facts but it tells you the ways in which our country is not doing so well, for instance the universities, and the ways it is doing much better. So, it is a very short sighted policy to prevent people from going elsewhere—going abroad.

In concluding, I want to improvise on something I just thought while hearing my other colleagues speak. This has to do with one's motivation for doing research. Torsten Wiesel and I are in the business of brain research, and one can say a lot about the potential and past uses of brain research. It is certainly clear that psychiatry has undergone a revolution in the last decade or so with drugs alone. The fact that half the beds in the country are occupied by

schizophrenics tells you that if you want to put money into something that will be valuable for the country, brain research is not a bad thing. I think that one would have to say that the real reason most of us do the work we do is not, to be quite honest, because of some pragmatic goal; the goals look after themselves. Torsten and I work on eye research and we found things that have to do with blindness that results from being crosseyed early in life, but we did not set out to do that. The reason we do what we do is curiosity—scientific curiosity—and probably that it is plainly and simply fun to do. After all, the brain is probably the most complicated machine in the universe; at least, it is the most complicated one that we know about and it is a challenge, and it is fun to ask how it gets the way it is and how it works.

The reason why we work, then, is not primarily pragmatic, it is because we like it. It is the same reason why a musician composes and why an astronomer looks through a telescope, if any astronomers do that any more. And, one senses in this country a kind of lack of national pride in that sort of activity, that we should focus all of our attention on the pragmatic value of these things and not just a fierce pride that one has when athletes win gold medals. When we have Nobel Prize Winners, when we have authors who succeed, or composers, these are ends in themselves and they are ends that are somehow neglected and one hates to see that. So, anything that our Government or anyone else can do to encourage these things, I think ought to be pushed by people like ourselves. One cannot help coming back from Stockholm and thinking that Americans are spoiled in the attitude of their country. Maybe we have had too many Nobel Prize winners, but other countries certainly make more of their Nobel Prize winners than the United States does. I think that is probably a good thing in some ways since it may go to all of our heads. But, we were not regaled with letters from all sorts of Congressmen and Senators, and Presidents, and people like that as our colleagues from other countries were. Our sense of values may, somehow, be at fault. One ought to be aware of that and be aware of the fact that when a child tries to figure out what he wants to do with his life, unless he knows that the resources are there, but particularly unless he knows that the country takes people like authors, musicians, and scientists seriously, we will continue to have a pausity of those kinds of people.

[The prepared statement of Dr. Hubel follows:]

Testimony of David H. Hubel

To the Committee on Science and Technology

In the past 20 years medical and biological research in the USA has taken the lead in the world. No other country can come close in quality or quantity. This at a time when we seem to have fallen behind Western Europe and Japan in just about everything else: cars, radio and TV sets, cameras, heavy industry, medical care, music, art, alleviation of poverty--practically everything except computers. In biomedical research we must have done something right. Are we in danger of faltering?

I think most people will agree that the single biggest reason for our success in biology and medicine is the National Institutes of Health, its system for supporting research, and its resources. Especially the system, which allows young people the freedom to get support and to work and direct their programs independently. The system of peer review by Study Sections works so well and is so fair that one shudders at hearing it from time to time challenged because it is expensive or time-consuming. The money is of course important, and the money is mostly efficiently used, and not for the most part wasted on administering or writing requests and reports. Scientists get the time to do their research. If anything is wrong with the system of support it is a lack of consistency. We go from times like the 60s in which it was hard to write a grant proposal so bad that it would not be funded, to a time like the present in which even the best scientists breathe sighs of relief when their grants are funded. The times of feast are bad because bad young scientists are trained and in turn train more bad scientists. In times of famine the system is such that good and bad both suffer, not just the bad, and the good scientists are not replenished. In science the most important resource is the scientist. It is essential to dampen things down in good times so that the weakest are not force-fed, and to bend over backwards in bad times to find the money to keep alive what we already have.

The success of our science, measured even by the crude and simple index of Nobel Prize winners has three main sources. The first, our enlightened policy and system of support, I've just dealt with. The second is our university system; the third, immigration of scientists and contacts with other countries.

Our university system is important not just because it does well at teaching. For scientists, universities provide a setting for research that is almost ideal. A department at a university is usually run in a loose democratic way so that even an assistant professor has complete independence in his research and a freedom from crushing teaching and administrative loads. His research money comes direct from the NIH, for all practical purposes, not through an autocratic chairman. The chairman's work and administration are shared among several senior people so that even he has time for his research. Compared with all this, the universities in Europe and Japan are disasters, so much so that the research tends to be done at institutes such as the Pasteur in France, the Max Planck in Germany or the MRC in England. Students pay a heavy price for this separation of teachers and scientists.

One hears more and more about the financial plight of American universities, especially private ones, in the USA today. They can't stay alive just by raising student fees. Speaking as a Harvard Professor who can't afford to send three boys to college, I can say for certain that fees cannot be raised much more. If Congress can do anything to make legislature that mitigates this plight, you will, among other things, have done a service to our science.

Finally, I don't think it is commonly realized to what degree science today is international. If the USA is to keep its leading position in science it must maintain free and extensive communication with other countries. We have much to learn, especially today from Japan and Western Europe. A scientist needs to know quickly when another laboratory discovers another method or breaks open a new field. Sharing means giving as well as getting, and we have much to give. Where we are beginning to fail most dismally, compared with Europe and Japan, is in sharing people.

It is becoming more and more difficult to bring well-trained and promising scientists into the USA, and to support trainees from abroad. (Of the five of us American Nobel Prize winners, could the three who were born and trained abroad have immigrated here if the restrictions were what they are today?) Our government looks with suspicion on anyone here who wants to go abroad for training. If a scientist asks to go to a meeting in Europe or Japan the first reaction seems to be that he wants a vacation, not that he might learn something valuable.

To summarize, I would make a plea for three things: more consistency in support of research, including training of scientists (especially since we seem to be on the verge of a famine); support for our universities, in a situation in which they seem on the verge of bankruptcy; and promotion of freer interchange with scientists abroad, with some money for travel and training both for Americans and foreigners, and an easing of immigration restrictions.

February 25, 1982

Chairman FUQUA. Thank you very much, Dr. Hubel, and to all of you for, I think, very interesting statements and very thought-provoking comments that you have made.

[The biographical sketch of Dr. Sperry follows:]

ROGER WOLCOTT SPERRY

Born in Hartford, Connecticut, August 20, 1913.
A.B.—Oberlin College, 1935.
M.A.—Oberlin College, 1937.
Ph. D. (Zoology)—University of Chicago, 1941.
Honorary Degree: D.S. from Cambridge University, 1942.
Assistant Professor of Anatomy, University of Chicago, 1946–52.
Assistant Professor of Psychology, University of Chicago, 1952–53.
Hixon Professor of Psychobiology, California Institute of Technology, 1954 to date.
National Research Council Fellow, Harvard University, 1941–42.
Research Fellow in Biology, Yerkes Laboratories on Primate Biology, 1942–46.
Section Chief, Neurological Diseases and Blindness, National Institute on Health, 1952–53.
Member, Experimental Psychology Study Section, 1966–70; Chairman, 1969–70.
Member, Fellowship Committee, National Science Foundation, 1963–64.
Member, Visitors Committee, Massachusetts Institute of Technology, 1969 to date.
Warren Medal, Society of Experimental Psychologists, 1969.
Distinguished Science Contributor Award, American Psychological Association, 1971.
California Scientist of the Year, California Museum of Science and Industry, 1972.
William Thomson Wakeman Research Award, National Paraplegia Foundation, 1972.
Certificate of Merit, Office of Scientific Research and Development, 1973.
Passans Award, 1973.
Member: National Academy of Sciences, AAAS (Fellow), American Academy of Arts and Sciences, American Psychological Association (Fellow), American Association of Anatomists.
Research Interests: Psycohobiology; neural plasticity tested by nerve and muscle transplantations; chemospecificity in growth of nerve connections; neural mecha-

nisms in learning and memory; split-brain approach to cerebral organization; mind-brain relations; hemispheric specialization.

Nobel Prizewinner in Medicine, 1981.

Chairman FUQUA. At this point in the record I ask unanimous consent that a letter from your fellow laureate, Dr. Roger Sperry of the California Institute of Technology be inserted. Dr. Sperry is unable to join us today, but we appreciate very much his interest in giving the committee his views, and we thank him for providing those in a letter addressed to me as chairman of the committee. Dr. Sperry also enclosed with his letter an article which we will also include in the record as part of the letter.

[The material referred to follows:]

STATEMENT OF DR. ROGER SPERRY, HIXON PROFESSOR OF PSYCHOBIOLOGY, CALIFORNIA INSTITUTE OF TECHNOLOGY, PASADENA, CALIF.

CALIFORNIA INSTITUTE OF TECHNOLOGY

PASADENA, CALIFORNIA 91125

DIVISION OF BIOLOGY 156-29

February 24, 1982

RECEIVED

Don Fuqua
Chairman
Committee on Science and Technology
U.S. House of Representatives
Suite 2321 Rayburn House Office Building
Washington, D. C. 20515

Dear Don Fuqua:

Thank you for your letter and invitation to submit a statement regarding federal support of science. I did not know the date of the hearing or would have sent this earlier.

I concentrate here on a matter of broad policy perspective rather than budgetary specifics in hope it may help add a new twist in the many reasons for giving stronger support to pure science and basic research.

If we continue to foster science and technology from just the usual immediate practical, utilitarian viewpoint, the result, at best, is that our advancing science and improved technology enable us to better maintain more people in better style for a while until the population numbers catch up, new limits are reached and the same problems reappear along with new ones and all on a greatly magnified scale. The end result is to put us in a self-feeding viscious spiral in which we get trapped deeper and deeper, year by year—with science and technology helping to reinforce and escalate the process.

The answer and strategic key to breaking these viscious spirals (aside from nuclear war, global famine, or other catastrophic breakdown), lies in achieving a change, world-wide, in mankind's sense of ultimate value, something that will reinforce more profound social priorities. This same conclusion is reached also by Lester Brown of the World-watch Institute. (See his new book "Building a Sustainable Society", Norton, 1981, especially the final chapter.)

It follows that no more critical task can be seen for the immediate future than that of seeking for civilized society a new elevated set of value guidelines more suited to man's expanding numbers and new powers over nature, a frame of reference for value priorities that will act to secure and conserve our world instead of destroying it. The great social need of our times is for a new value-belief system or ethic that will make it "sacrilegious" to overpopulate, to pollute, to deplete irreplaceable resources, or in any other way to destroy, despoil, or desecrate the quality of the biosphere for coming generations. More than any other causal system with which science now concerns itself, it is these variables in human value systems that will determine the future.

The place where science fits into all this is in regard to the question: "How do we get there, i.e., to a new remedial set of value guidelines?" There are many strong reasons to believe that we must look to science to play a major role. Values and beliefs to live and govern by are best founded on the kinds of truth, worldview perspectives, and valid ultimate criteria provided by the scientific method. Science approaches closest, among all the available alternatives, to furnishing what is most needed for value judgments, namely, a consensus on some supreme interpretation of the universe and the role within it of man and the life experience. Science can be further valued in this context as a means to deeper and more intimate understanding of the forces that made and move the universe and created man.

Everything suggests that society would benefit greatly in our present period of confusion and uncertaintly, by putting a little more faith in science and promoting science into a higher cultural role as a prime source of improved value-belief guidelines.

The perspectives from which to judge federal support of science shift correspondingly to include higher concerns for the ultimate criteria and frame of reference for moral priority. It would mean a greater emphasis on pure science and basic research, particularly in such areas as mind-brain science, cosmology, nuclear physics, behavioral science and whatever bears most directly on the shaping of higher, long term values.

All these arguments start from and take for granted, the new changed concepts of mind and brain that have brought a revised outlook in science in which mind transcends and controls matter. The natural world and human psyche are no longer perceived to be reducible to quantum mechanics. On these new terms science no longer demeans or dehumanizes. What science stands for, its scope, limitations, worldviewperspectives and its role as an intellectual and cultural force are fundamentally reformed as is the position of science in respect to values and moral judgment.

The traditional teaching that science and value questions belong in separate realms and that the one must not be applied to the other are reversed. Mergence of the two is not only feasible but natural and urgently needed above all other priorities as the prime hope for tomorrow's world, offering the strategic key to breaking the viscious spirals of worsening world conditions.

For any who may be interested, the back-up reasoning for these arguments is better expressed in the enclosed recent article and its references.

Yours respectfully,

Roger Sperry

Roger Sperry

RS:skm
Enclosure

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CHANGING PRIORITIES

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Roger W. Sperry[1]

Division of Biology, California Institute of Technology, Pasadena, California 91125

INTRODUCTION

Rather than address select aspects of neuroscience, or particular problems with which I happen to have been involved, or to personally reminisce about golden "days that are no more," I plan to respond to the kind invitation to write a prefatory chapter for this volume with a few observations bearing on matters that, while personal in some respects, are also of general ongoing concern to all of us in science and particularly to the future of neuroscience as a discipline.

We used to say that there are two kinds of scientists: those fired-up by a problem and searching for methods to get the answers, and those highly trained in some method who are searching about for some amenable problems. While most of us line up somewhere between these extremes, there is much to be said for, at least in principle, giving preference where possible to problem priorities over methodology. What follows is, above all, problem-oriented and attuned throughout to the query, "What difference does it make?—especially ten, twenty, or more years from now?"

In terms of governmental funding and in other respects, it has become apparent that the overall federal rating for neuroscience is not as high today as it was prior to the Nixon budget reforms of the early 1970s. Nor does it appear that the decline is something temporary from which funding can be expected soon to recover. Nor is it restricted to neuroscience; science in general has been affected with certain exceptions such as cancer-related, energy-related, and other select projects where a major application to current quality-of-life problems is obvious. These changes in relative funding can be assumed to reflect real changes in social priorities and in society's

[1]Preparation of the manuscript was aided by the F. P. Hixon Fund of the California Institute of Technology.

collective judgment of the importance of science and what it contributes. We read in *Science* (Sawhill 1979) of the "public disillusionment" and "today's more jaundiced view" of science and that "faith in the beneficence of scientific endeavor and the promise of technology has been steadily eroding."

An underlying cause for these changes can be seen in the new and growing recognition of mounting global, so-called "crisis" problems which science is alleged to have helped to create and which in addition are complicated by social value problems for which science is apparently unable to provide answers. When the quality and even survival of civilized society is potentially in threat, what difference does it make to Congress or the public whether we find some new nerve connections in the brain, some new transmitters or receptors, and so on? Even the ever-strong humanitarian appeal of medical advancements that might eventually save hundreds of thousands of lives does not fully escape the new unspoken perspective of a world already afflicted with population imbalances in the hundreds of millions. The overwhelming priority of the growing demands of today's "global crisis" problems was already perceived in the late 1960s (Platt 1969) to be of sufficient magnitude and urgency to warrant mustering the scientific community in an all-out crash attack with the implication that to continue the practice of "science as usual" is morally indefensible.

Although little has happened in the interim to reduce the specter of global breakdowns, it seems that considerable has happened to discourage public feeling that science and improved technology can be counted on to bring solutions. While the world's production per capita of most major products of the basic biological source systems has already peaked and started on a long downward trend (Brown 1979), world demand continues to rise at the rate of 6 million people per month with predictions of inevitable social turmoil as peoples and nations grow more desperate.

Earlier hopes that science might rise to the occasion with "green revolutions" and other technological answers begin to fade. Science and improved technology, we come to realize, merely make it possible to better maintain more people in better style, for a while, until new limits are reached and the same problems reappear, along with new ones, and all on a greatly magnified scale. Science and advanced technology, whether medical, agricultural, military, energy-related, etc, are now seen, in the long run, to put us in an escalating vicious spiral of technology-population-energy-pollution-etc increases, in which we are now firmly entrapped. There is no reflection here on science or technology per se, of course. As we say, Utopia is tomorrow's level of technology combined with the population levels of the nineteenth century. Remedial suggestions, however, that might in any way involve the highly sensitive, value-laden factor of population controls

promptly raise a host of moral issues and value conflicts for which, again, science, it is held, does not provide answers.

Futurists and common logic concur that a substantial change, worldwide, in lifestyle and moral guidelines will soon become an absolute necessity. On a planet of finite resources, the laws and mores of a freely increasing population must eventually be replaced by those of a regulated population, and the sooner this inevitable shift occurs, the better for the residual quality of the biosphere. In brief, it becomes increasingly evident that the prime, urgent need of our times is not for more science and improved technology, medical or otherwise, but for some new ethical policies and moral guidelines to live and govern by.

Once this conclusion is perceived, the common tendency is to by-pass science to look elsewhere for answers. Problems that resolve to basic issues in ethics and morality are traditionally supposed to be beyond science. It is argued that science which describes facts cannot be used to prescribe values. Prevailing doctrine in modern philosophy asserts that it is logically impossible to derive values from a set of scientific facts, or to infer what ought to be from what is. In view of the collective effect of various considerations of this kind, it is hardly surprising that public faith in the promise of science and technology "has been steadily eroding."

A DIFFERENT APPROACH

In what follows I try to defend a position directly counter to the above, which would, in effect, not only restore to science any loss in public favor, but would go further to give science, and the scientific endeavor generally, a new public image and a higher societal role of top priority. On the proposed terms, science becomes the prime hope for escape from the vicious spirals of advancing civilization, but for other reasons. A different approach to the public support and role of science is suggested in which science is upheld, not because it begets improved technology, but because of its unmatched potential for the shaping of ethical values. In the worldview perspectives and truths of science we will find the best key to valid moral guidelines. The arguments are adapted to today's priorities and grow stronger, not weaker, as current global conditions worsen. Even basic "pure" research and the practice of "science as usual" emerge on the proposed terms with a heightened social and moral approval.

The usual appeal to medical, educational, technological, and other direct benefits is by-passed and our bets are placed instead on certain less obvious human value implications that stem from brain research. Particularly relevant are recent changes in concepts relating to the mind of man, the nature of the conscious self, freedom of choice, causal determinacy, and to the

fundamental relation of mind to matter and to brain mechanism. Some of man's most enduring concerns are involved, i.e. whether consciousness is mortal or immortal, cosmic or brain-bound, or reincarnate, and the like. It is in terms of the humanistic implications along these and related lines that neuroscience has always had its special interest and greatest meaning. Ideologies, philosophies, religious doctrines, world-models, value systems, and the like will stand or fall depending on the kinds of answers that brain research eventually reveals. It all comes together in the brain.

In brief, recent conceptual developments in the mind-brain sciences are seen to bring changes in worldview perspectives that revise the ultimate criteria and frame of reference for determining human value priorities and resolving value differences. A broad shift of conceptual framework regarding science and human values is involved. Promising prospects can be seen, especially as these changed perspectives apply in those global crisis areas wherein lie today's most serious threats to the quality of life, and where differential outcomes in the resolution of value conflicts tend to have tremendous social consequences. For example, even a slight shift in the delicate balance of value perspectives on the abortion vs right-to-life issue could mean the difference, in itself, of literally many millions of lives, pro or con, in the next few years—with enormous secondary impacts as well—and all compounded on future generations. Similar wide-ranging, quality-of-life consequences result from a shift of values in regard to energy-environmentalist issues, species' rights, and other global concerns.

It is our present contention that a scientific approach to both the theory and the prescription of ethical values is not only feasible, but is by far the best way to go, offering the most promising, perhaps only, visible hope for future generations. The supporting arguments have already been expounded in some detail elsewhere and may be found in the original articles cited and their references (Sperry 1965, 1969, 1972, 1977, 1980). Rather than assume prior knowledge or laboriously restate the reasoning in different words, it is more expedient for present purposes to simply list below some of the principal postulates, propositions, observations, and inferences as excerpted with minor changes from the previous accounts. Because the subject matter ranges somewhat afield from neuroscience, overlap and redundancy are risked rather than the reverse. Some attempt at logical ordering is maintained, but the cross logistics mount rapidly, and a quick grasp of the whole may be found preferable to a logical sequential approach.

COLLECTIVE POSTULATES AND PROPOSITIONS

Subjective Values in Objective Perspective

•In addition to their commonly recognized significance from a personal, religious, or philosophic standpoint, human values can also be viewed objec-

tively in causal control terms as universal determinants in all human decision-making. All decisions boil down to a choice among alternatives of what is most valued, and are determined by the particular value system that prevails.

•Human values, viewed in objective, scientific perspective, stand out as the most strategically powerful causal control force now shaping world events. More than any other causal system with which science now concerns itself, it is variables in human value systems that will determine the future.

•Any given brain will respond differently to the same input and will tend to process the same information into quite diverse behavioral channels depending on its particular system of value priorities. In short, what an individual or a society values, determines very largely what it does.

•As a social problem, human values can be rated above the more tangible global concerns such as those of poverty, pollution, energy, and overpopulation on the ground that these more concrete problems are all man-made and are very largely products of human values. Further, they are not correctable on any long-term basis without effecting adaptive changes in the underlying human values involved.

•The human value factor in biospheric controls stands out as the primary underlying root cause of most of today's difficulties. The more strategic way to remedy mounting adverse global conditions is to go after the social value priorities directly in advance, rather than waiting for man's values to change in response to changing external conditions. Otherwise we are doomed to live always on the margins of intolerability, for it is not until things begin to get intolerable that the voting majority gets around to changing its established values.

•Recent developments in the mind-brain sciences eliminate the traditional dichotomy between science and values and support a revised philosophy in which modern science becomes the most effective and reliable means available for determining valid criteria for moral value and meaning.

Value Theory

•A science of values in the context of decision theory becomes conceivable extending into all branches of behavioral science and forming a skeletal core for the social sciences.

•The seemingly endless complexity of human values is greatly simplified by viewing values in hierarchic structures which are goal dependent, and further, by restricting attention to those areas that are involved in social conflict.

•The innate components of the value structure, which include inherent psychological as well as biological values, can be treated largely as a common invariant denominator of human nature, allowing the focus of atten-

tion to be directed to problems of the acquired, cognitive, ideological values where the major sources of value conflict arise.

•On analysis, values are found to be correlates of directed activity. They are always relative to some purpose, goal, or aim, explicit or implicit, and structured in goal-dependent hierarchies. Any concept or belief regarding the purpose and value of life as a whole, once accepted, then logically supersedes and conditions the entire hierarchy of value priorities at subsidiary levels. Values at the ideological plane become ordered and ethical issues judged in accordance with the conceived ultimate purpose of life as a whole. This latter will logically imply at the same time an associated worldview or universe scheme that is consistent.

•Because of the hierarchic structure of values the search for improved ethical guidelines can be narrowed to the search for what ought to be most valued. This in turn leads to problems of the highest determinants of value priorities—the "life goal," "world model" concepts and beliefs that lie at the heart of the problem of moral judgment and logically condition the value structure at all levels.

•Societal values, especially of the kind people disagree on, are always dependent upon, and relative to, some general frame of reference containing the premises, beliefs, presuppositions, etc on which the reasoning about priorities rests. The question may be raised "What makes one reference frame superior or supersedent to another?" and then, "Is there some ultimate frame of reference for values that could logically and rightly be accepted and respected by all countries, cultures, governments, and creeds, and by mankind in general, as the final supreme standard when it comes to judging ethical priorities, resolving value conflicts, and as a guideline for human judgment generally and international decision-making in particular?" The practical need for some such unifying global standard becomes more and more evident for things such as world population control, conserving world resources, protecting the oceans and atmosphere, and for various other modern world problems that increasingly require united effort on a global scale.

•What is needed ideally to make decisions involving value judgments is a consensus on some supreme comprehension and interpretation of the universe and the place and role within it of man and the life experience.

Dependence on Mind-Brain Concepts

•Beliefs concerning the ultimate purpose and meaning of life and the accompanying worldview perspectives that mold beliefs of right and wrong are critically dependent, directly or by implication, on concepts regarding the conscious self and the mind-brain relation and the kinds of life goals and cosmic views which these allow. Directly and indirectly social values depend, for example, on whether consciousness is believed to be mortal,

immortal, reincarnate, or cosmic and whether consciousness is conceived to be localized and brain-bound or essentially universal, etc.

•Recent developments in mind-brain theory revise the ultimate criteria and our ultimate frame of reference for determining value priorities. Problems of values, ethics, and morality (questions, i.e., of what is good, right, and ethically true and of what ought to be) become, in these revised terms, something to which science, in the most profound sense, can contribute fundamentally and in which science should be actively and responsibly involved.

•Current concepts of the mind-brain relation involve a direct break with the long-established materialist and behaviorist doctrine that has dominated neuroscience for many decades. Instead of renouncing or ignoring consciousness, the new interpretation gives full recognition to the primacy of inner conscious awareness as a causal reality.

•The phenomena of conscious experience are conceived to play an active, directive role in shaping the flow pattern of cerebral excitation. Instead of being parallelistic and acausal, consciousness in the present scheme becomes an integral part of the brain process itself and an essential and potent constituent of the action. Consciousness is put to work and given a use and a reason for having been evolved in a physical system. Subjective phenomena including values are brought into the causal sequence in human decision-making and behavior generally and thus back into the realm of experimental science from which they had long been excluded.

•The seemingly irreconcilable dichotomies and paradoxes that formerly prevailed with respect to mind vs matter, determinism vs free will, and objective fact vs subjective value become reconciled today in a single comprehensive and unifying view of mind, brain, and man in nature.

•The swing in psychology and neuroscience away from materialism, reductionism, and mechanistic determinism toward a new, monist, mentalist paradigm restores to the scientific image of human nature the dignity, freedom, responsibility, and other humanistic attributes of which it has long been deprived in the materialist-behaviorist approach.

•A nonreductive holistic world model and interpretation of physical reality is supported in which the qualitative pattern properties of all entities are conceived to be just as real and causally potent as those of their components. This preservation of the qualitative value and pluralistic richness of physical reality stands counter to the common tendency to correlate science with reductionism.

Toward the Prescription of Values

•Instead of separating science from values, the current interpretation leads to a stand in which science becomes the best source, method, and

authority for determining the ultimate criteria of moral value and those ultimate ethical axioms and guideline beliefs to live and govern by.

•The classic *fact-value* and *is-ought* dichotomies of philosophy logically dissolve in the context of cerebral processing. The operations of the brain are already by nature richly replete with established values and value determinants, both inherent and acquired, with the result that incoming facts regularly interact with and shape values. The resultant value system, along with conceptions of what ought to be, is determined in very large part by the factual input.

•Changing to an ethic based in science would entail in large part a substitution of the natural cosmos of science for the different mythological, intuitive, mystical, or "other-worldly" frames of reference by which man has variously tried to live and find meaning. The aim is not to eliminate value controversy and differences of opinion but only to bring these into a domain set by an agreed-upon frame of reference supported by science—not with the idea that scientific truth is absolute or beyond question, but only with a conviction that it does represent the best and most reliable, credible, and dependable approach to truth available.

•Once science modifies its traditional materialist-behaviorist stance and begins to accept in theory and to encompass in principle within its causal domain the whole world of inner, conscious, subjective experience (the world of the humanities), then the very nature of science itself is changed. The change is not in the basic methodology or procedures, of course, but in the scope and content of science and in its limitations, in its relation to the humanities and in its role as a cultural, intellectual, and moral force. The kinds of interpretations that science supports, the world picture and attendant value perspectives and priorities, and the concepts of physical reality that derive from science all undergo substantial revisions on these new terms. The change is away from the mechanistic, deterministic, and reductionistic doctrines of pre-1965 science to the more humanistic interpretations of the 1970s. Our current views are more mentalistic, holistic, and subjectivist. They give more freedom in that they reduce the restrictions of mechanistic determinism, and they are more rich in value and meaning.

•Accepting as self-evident the ultimate value of what man generally has held most sacred, namely, the cosmic forces that made, move, and control the universe and created man, and interpreting these in accordance with science, one emerges with a value system that includes a strong reverence for nature promoting the values of the recycle philosophy, population regulation, protecting and enhancing environmental quality, and the like.

•In the eyes of science, to put it simply, man's creator becomes the vast interwoven fabric of all evolving nature, a tremendously complex concept that includes all the immutable and emergent forces of cosmic causation that control everything from high-energy subnuclear particles to galaxies,

not forgetting the causal properties that govern brain function and behavior at individual and social levels. For all of these, science has gradually become our accepted authority, offering a cosmic scheme that renders most others simplistic by comparison and which grows and evolves as science advances.

•Science becomes man's best channel for gaining an intimate understanding of and rapport with those forces that control the universe and created man. This is not to suggest that science take on the functions of religion; but only that there might be mutual and other benefits from a fusion of the two.

•The future of science will be very different depending on whether science is recognized in the public mind to have competence in the realm of values. Reciprocally the future of society also will be very different depending on whether its value perspectives are shaped from science and the worldview of science or by other alternatives that now prevail.

THE KEY TO QUALITY SURVIVAL

Implicit in the foregoing is the conclusion that our top social priority today is to effect a change worldwide in man's sense of value. This translates by hierarchic value theory into a change in what is held most sacred. What is needed, more specifically, is a new ethic, ideology, or theology that will make it sacrilegious to deplete natural resources, to pollute the environment, to overpopulate, to erase or degrade other species, or to otherwise destroy or defile the evolving quality of the biosphere. This is exactly what is found to emerge from our current approach to the theory and prescription of human values. Relying on the kind of truth supported by science we arrive at an ethic that promotes an ultimate respect for nature and its creative principles, including those of its peak thrust into the highest reaches of man's mind, along with corollary value criteria which, if applied worldwide, would promptly set in motion the kinds of corrective legislation and other trends and pressures that are needed to remedy looming global disaster conditions.

On the terms proposed the utility of science takes a different form. Society would look to science not only for new technology and objective knowledge but, more importantly, for the criteria of ultimate value and meaning. Each advance of science brings increased comprehension and appreciation of the nature, meaning, and wonder of the creative forces that move the cosmos and produced man. Even "science as usual" gains, in this context, a heightened social significance and moral support. The special, key role of neuroscience and brain research will be readily apparent.

It remains to further stress a point already implied, namely, that for science to fully qualify and function effectively in this changed role, it will be necessary that we abandon an entire mode of scientific thinking long

referred to under the general rubric of "scientific materialism." Moves to abide by the truths of science, as opposed to unproven claims from other sources, have had sporadic support since Francis Bacon. What is new today is the shift in science from reductive physicalism to a holist-mentalist paradigm and the changed interpretations and perspectives that this brings. Among traditional views that consequently require correction is that predicating the impotence of science in regard to value judgments along with much of the doctrine associated with reductive mechanistic determinism that for many decades has characterized science and our scientific outlook. This was the thinking of Karl Marx and is the reason that the more materialistic and animalistic aspects of human nature are put first in Soviet philosophy before man's more idealistic, more spiritual components. The issues at stake are not minor. They involve not only the public image of science, the relation of science to human values, and the kinds of values science upholds, but also some of our more basic concepts in science concerning physical reality, mind and matter, and the nature of causation.

CAUSAL DETERMINISM: THE CENTRAL ISSUE

The issues narrow to opposed views of causation that are basic and central to everything stated thus far. One view holds that the causal forces and laws operating in nature are fully explainable in purely physical terms and are, in principle, ultimately accountable on the basis of quantum theory, the elemental forces of physics, or in some more unifying field theory eventually to be found. Physicalist, i.e. materialist, determinism is assumed to prevail throughout nature in this view and all higher level interactions, including those of the brain, are presumed to be reducible and accountable, in principle, in terms of the ultimate fundamental forces of physics.

Opposed to this long dominant physicalist-behaviorist interpretation is the view which we here uphold and which has recently been gaining increased acceptance particularly in the behavioral sciences. It contends that the higher forces and laws of causation, as seen for example in classical mechanics, in physiology, and in brain function and behavior, cannot be fully explained by the laws of quantum mechanics or by the mechanics or laws of any other ultimate physical force or field. The higher entities and their causal properties and laws of interaction are conceived to be causal realities in their own right and not determined completely (though they are in part) by the causal laws and properties of their components. The larger, higher, more molar properties are perceived to be just as real, just as causal, and in many ways to be of more importance than are the more basic physical properties of their subsidiary components. In this view the fundamental forces of physics are only building blocks used in creating bigger, more competent entities and forces. The patterning of the building components,

i.e. their arrangement in space and time, becomes a distinctive key factor in making things what they are, and is not determined solely by the properties of the parts themselves.

To attempt to explain an entity in terms of its parts and then the parts in terms of their parts and so on, results in an infinite regress in which one is left at the end trying to explain everything in terms of next-to-nothing. At each step of the way critical pattern components of causality are lost and the explanation becomes less and less complete at each lower level. To attempt to include, even in principle, the pattern factors, i.e. the space-time components, by invoking the "interactions of the parts," the "organizational relations," etc at each step, sounds good but is an empty lip service. We have no science for the space-time components, no science of the organizational relations and interactions. Particularly we have no science for the collective form in which these are present at each level of the infrastructure. Even the relatively ultra-simple interactions involved in the classic "three-bodies problem" are too much.

Our present view holds further that when a new entity is created the new properties of the entity, or system as a whole, thereafter overpower the causal forces of the component entities at all the successively lower levels in the multi-nested hierarchies of the new infrastructure. In other words whenever an entity joins forces with others to form a new whole, the position that it is forced to take in the universe and its subsequent course through time and space and its eventual fate are thereafter determined more conspicuously by the new properties of the system as a whole than by its own original properties. A degree of self-determinacy is lost to the parts as soon as the higher powers of the new whole become superimposed. Although the causal forces at the lower quantal, atomic, molecular, etc levels in the infrastructure continue to operate in full force as usual they are enveloped, encompassed, overwhelmed, superseded, supervened, and outclassed by the new causal properties that emerge in the whole. Evolution, in the course of compounding new compounds continuously adds new entities and new phenomena that embody new qualities, new causal forces, and principles with new scientific laws and control properties.

The new emergent phenomena, not reducible, in principle, to their parts and deserving to be recognized as causal realities in their own right, are in many respects more powerful and dominant features of reality than are the lower properties of the components. Instead of a universe completely controlled by quantum mechanics and the basic forces of physics, science presents, by this interpretation, a universe controlled by a rich profusion of qualitatively diverse emergent powers that become increasingly complex and competent. Any randomness, chance, caprice, or chaos that may be operating at the quantum level, as modern physics insists, gains little expression because it is effectively superseded and controlled by higher level forces

that are anything but random. The higher levels involve much more than mass probabilities. The creative, interlocking web of evolving nature is not blind or chance-like but becomes, as it progresses, rich in irreversible, directional, ever more complex constraints that tend to keep things moving in a trend toward higher and more competent forms.

In the brain, controls at the physico-chemical and physiological levels are superseded by new forms of causal control that emerge at the level of conscious mental processing, where causal properties include the contents of subjective experience. Causal control is thus shifted in brain dynamics from levels of pure physical, physiological, or material determinancy to levels of mental, cognitive, conscious, or subjective determinacy. The flow of nerve impulse traffic and related physiological events in a conscious process is no longer regulated solely by events in kind but becomes caught up in, enveloped, and moved by the higher mental controls, somewhat as the flow of electrons in a television set is moved and differentially patterned by the program content on different channels. Just as the programming variables of a TV monitor have to be included in order to account for the electron flow pattern of the system, so also in the brain the subjective, mental variables of cerebral function have to be included to give a full account of the flow patterns of neural excitation. The mental events of conscious experience and the physico-chemical events of the infrastructure are not conceived to be in a parallelistic relation like that of "two languages," "two logics," or of "two complementary aspects of one and the same situation" in which a "purely physical determinacy of the CNS" is preserved as MacKay (1980) and others would have it. This shift from a causal determinacy that is purely physical to one that includes conscious subjective forces that supersede the physical—in other words the shift from a materialist, reductionist, mechanistic paradigm to a holistic, mentalist paradigm—makes all the difference when it comes to using the "truths" of science as criteria of ethical values.

MARXISM INVERTED

In trying to assess possible social repercussions and the outcome of a societal shift to an ethic founded in science, it would be unfortunate and misleading if one were to rely on Marxism and the Communist World as an example. According to our latest mind-brain theory and its implications, Marxist-Communist doctrine is founded on some basic errors in the interpretation of science and of what science stands for and implies in reference to human nature and to social and worldview perspectives. As a result, the kinds of values upheld in Marxist doctrine are almost the diametric opposite

from those which emerge from a scientific approach on our present terms.

If the growing competition between Communist and Free World countries is to continue to be in part a battle for men's minds, it may be worth a few words in closing to point out some of the ideologic value differences that result even though the intent in both cases is to exclude dualist "otherworldly" criteria in favor of the truths of science. Some of the basic starting differences include the following:

1. First and foremost, Marx accepted the long established—but now largely overthrown—view that science, of necessity, leads to and supports a materialist philosophy that rejects subjective mental phenomena as causal and predicates instead a purely materialistic determinancy for the CNS.
2. The doctrine of reductionism was accepted as applied to nature in general and human nature in particular.
3. Also intimately related to the above, Marx failed to recognize the key principle of "downward causation," i.e. the causal control that higher properties in any entity, whether a society or a molecule, invariably impose over the lower properties of their infrastructure.
4. Marxism, further, lacks any theory that serves to resolve the *is-ought* fallacy or the traditional dichotomy that has heretofore kept science and human values separate.
5. Marxism also lacks the "free-will" concepts we have today that free individual and social decision-making from mechanistic determinism.
6. Marx opted for a throughly homocentric value system that makes man the measure of all things and gives precedence to the basic material needs of man over the quality of the biosphere, as well as over man's higher psychological needs. There is no justification in science for this latter choice and it is, in some respects, an inversion of nature that puts the welfare of a part of a system above that of the system as a whole.

Basically, according to Marx, what counts in human affairs and changes the world are man's actions in fulfilling his material needs for subsistence, not man's idealisms, philosophy, or ideology. He emphasized that the materialistic-animalistic needs must come first and that the higher human pursuits are built upon and depend on the more basic components. On the other hand, Marx failed to appreciate that the higher idealistic properties in man and society, once evolved, can then supersede, encompass, control, and take care of the lower material needs; that this is the way of evolving nature and when it comes to rules for progress, works better than the inverse. One of the best refutations of Marxism is Marxism itself in that it was not Marx's actions in satisfying his material needs for subsistence that changed the world, but his philosophy, visionary ideas, and Communist ideology.

A value system that puts its ultimate good in the welfare of the "Party," and at the same time pointedly scorns reverence for nature, does little to help remedy most of our mounting global crisis conditions which today are the overriding concern. There is nothing in Marxist doctrine to help control overpopulation, curb pollution, conserve resources, preserve the environment, protect endangered species, etc. Nature in Marxist materialism is not something to be revered but almost the reverse, i.e. something to be battled and subjugated, transformed, mechanized, and exploited to satisfy man's (mainly material) needs. The forces of nature as interpreted by Marx in the materialist tradition are blind and unprincipled (Bell 1975), not something rich in quality, wonder, and beauty, harmoniously controlled with countless checks and balances, and full of creative strategies, constraints, and principles that have been time-tested for success in creating, preserving, and improving the quality of the biosphere, including man.

In Marxism not nature but technology and production power are idolized. Cathedrals for Marx are factories and skyscrapers, and the beautiful dream is to transform whole continents by industrial progress with "huge new populations springing up as if by magic" (Ryanzanoff 1963). The narrow focus on class conflict in an industrialized society also does nothing for the major ailments of the planet today and again is expressed in terms of the mechanistic determinism of the more material and elemental needs and components in man's makeup at the expense of the higher psychological needs and more idealistic components. The causal power of cognitive ideals that behavioral science recognizes today, was, on principle, dismissed. Where nothing is sacred and there is no higher meaning (beyond that of the "Party"), everything loses meaning. In its homocentric emphasis on the more material needs and aims of man, technology, industry, and production power, combined with its demeaning view of nature, Marxism, in direct contrast to the views we reach above, seems to represent almost the epitome of the worst forces that have caught up with us today to produce most of the adverse crisis conditions that threaten the future. The basic relevance of brain research to all these issues can hardly be overemphasized.

IN SUMMARY

In the context of today's mounting global problems the relative demand for medical, educational, and related social benefits that derive from the neurosciences is diminished. At the same time the human value spin-offs of brain research are thrust into a strategic position of top concern because of their key role as criteria for policy priorities and decision-making guidelines. Recent conceptual developments in the mind-brain sciences rejecting reduc-

tionism and mechanistic determinism on the one side, and dualisms on the other, clear the way for a rational approach to the theory and prescription of values and to a natural fusion of science and religion. Science can be upheld as the best route to an increased understanding and rapport with the forces that made and move the universe and created man. The outlines of a value-belief system emerge that include an ultimate respect for nature and the evolving quality of the biosphere, which, if implemented, would set in motion the kind of social change needed to lead us out of the viscious spirals of increasing population, pollution, poverty, energy demands, etc. The strategic importance of neuroscience and the central role of prevailing concepts of the mind-brain relation to all of the foregoing remain evident throughout, as does also the direct relevance of efforts to bring added insight and substantiation of these mind-brain concepts through further advances in brain research.

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Chairman FUQUA. Several of you mentioned—Dr. Hoffman and Dr. Wiesel, and maybe some of the others of you mentioned—the science education and the number of people that are taking calculus in high schools, and so forth. There was a recent article in one of our major magazines, just in the last few weeks. I think you called it a tragedy for science and math that a large number of people teaching math in the high schools who are not even certified as math teachers. Over the years, there has been a large amount of money to try to promote science education and maybe we have gone about it in the wrong direction. It seems that instead of getting better, we are getting worse. Do you have any comment about that and do you see that as a real problem facing the future of this country, not only for students that may be coming along—I think we still have many, many bright minds that are still in high school and in colleges—but when you see the large numbers that cannot do medium difficult division and other types of uncomplicated mathematics, does that cause you a great deal of alarm? What do you see as some solution to that problem? Apparently, what we have been doing has not worked.

Dr. HOFFMAN. Well, there are many good things about the American system of education. It provides very different university systems, college systems, for people to pursue their education. It does not exclude young people. I do not think we have done that badly by the teaching of basic mathematics. I think, as I mentioned in one sentence of what I said, teachers need something to get excited about to revive their skills in some way and their enthusiasm in what they are teaching. If they are going to be giving the same material three or four times a day for 20 years, they and you would get tired of that. I think some new way of approaching the teaching—a new set of textbooks at various institutes—can revive the interest of the mathematics teachers, as it can of the other science teachers. Another depression would not hurt. Some of the best teachers that I had were people who were forced to go into teaching because of the Great Depression; a rather cynical comment on what people had to do, but that is how I had my best teachers. Some of them had doctorate degrees because they could not get jobs elsewhere and could get them in the teaching profession.

Dr. SCHAWLOW. I feel more concerned than what, I think, you are expressing. I agree with the Chairman that there is a real problem. We have not many high school teachers, in fact fewer all the time, who are even minimally qualified to teach physics. I think that in view of the fact that the teaching profession is rather highly structured and there is not much turnover to be expected, we are going to have to make do with the teachers we have who, by and large, are people who would like to do a good job. It seems to me that one thing we would have to do is provide some method for them to upgrade their skills and knowledge of the subject, perhaps by making more use of summer institutes. I think some of the programs that the scientists and mathematicians have tried to introduce for producing a new approach to the subject have failed miserably because they have scared the teachers and the teachers just really do not know what to make of them. I would hope that one could find either a weekend institute, or summer institutes that could help the teachers to get some knowledge of the state of the

subject and some sense of the excitement of it while they are still performing their jobs.

Chairman FUQUA. Anyone else for comment?

Dr. HUBEL. Well, I guess that people go into the fields where they think society rewards them, either with money or prestige, or some kind of high regard. My own feeling is that people do not—the very best minds do not go into teaching. They go into areas where they will be rewarded.

I was on the admissions committee of Harvard Medical School and kids would come in and I would ask them why do you want to go into medicine? They would reply because they liked people. I would say, "why do you not go into teaching?" and there was sort of a dead silence. You realize that they like people but they also like medicine as a profession which is regarded by society as having a high status. Countries in Europe, I guess West Germany would be an example, call high school teachers "professor" and they are regarded as a kind of a national treasure. If we do not look upon teachers as being terribly important people, then we will not get good teachers.

Chairman FUQUA. Dr. Wiesel, you mentioned in your prepared statement that we were still leading in biomedical education. While we may have suffered in other areas, that is one we still lead in. Are we being threatened in this area? When I say threatened, I mean are we losing our position?

Dr. WIESEL. I do not think we are losing it. I think we are being threatened, looking in the future. Right now, I do not think we are threatened. I think a decrease of the flow of scientists to this country could be a threat. As was pointed out by David Hubel, this influx has been an asset to this country, and as Dr. Hoffman indicated it was very important to the rapid buildup of the science establishment in this country. That flow has now actually slowed down and some prominent American scientists have moved out of the country and established laboratories in other countries because their resources are better. I do not think that this is going to be a major outflux but I am using this as an indication of the fact that you are not doing a job quite properly here in terms of realizing the need to equip our laboratories better. We used to have a specific grant that Congress allocated for upgrading university equipment but that was canceled a year or so ago and we all look upon that as a big mistake. It is impossible for individuals to get sufficient money to buy equipment and so we are stalled here.

Chairman FUQUA. Dr. Schawlow, you mentioned the increase in productivity, your own productivity, when you were at Bell Labs and able to acquire new and improved equipment. Someone mentioned the third-level computers and also to have universities utilize computers to develop a better state of the art. Are you finding that is a real problem now? Is the obsolete equipment a very major problem? You are nodding your head, Dr. Bloembergen.

Dr. BLOEMBERGEN. I would agree with that. I do not think it is particularly computers that limit us.

Chairman FUQUA. I meant the whole field of instrumentation.

Dr. BLOEMBERGEN. That is right. It is different for different fields, but instrumentation in the United States universities is a

stepchild and has been a stepchild for many years and it ought to be corrected.

Dr. SCHAWLOW. I think we can see from the laser spectrocity that we do. We used to build our own lasers. Now we buy them at a cost of between $80,000 to $90,000 for a good, high-quality, tunable laser. It has got a lot of details in there that would take us years to catch up with, and it is not easy to get a new one of those.

Chairman FUQUA. Let me just say—you may want to count it as a post script to that question—what can we do about it?

Dr. BLOEMBERGEN. There are some precedents in the past. In the sixties there were one-time infusions of funds for instrumentation and technical support. A very notable one was for the Advanced Research Project Agency material research laboratories takeover in the mid-seventies by the National Science Foundation. But, there was a very significant infusion in the field of materials research.

A second example was an infusion for instrumentation funds from agencies of the Department of Defense in the late sixties and there had also been talk about it during the seventies but nothing has materialized.

Dr. HOFFMAN. There are a couple of things. One is that we get funds for instrumentation—and, incidentally, I think all of us at this table, represent people with instrumentation needs which are medium sized. None of us are working with big accelerators, with very large machines. Even in the case of the largest ones, I do not think the costs exceed, let us say, the level of $200,000, or something of that size, and certainly they are much less in the case of the simple equipment used by my biomedical colleagues. I depend very heavily on computers. It is the flow of that kind of instrumentation which is extremely important. Also, there is a category of expenses which is driving our universities to extremes and that is that even when we are provided with instruments, there seems to be something written into the granting agencies' mandates that prevents them from giving us money for upkeep. We are very good at salvaging old equipment. You should come around and see what we can do with ancient pieces of equipment. We keep them going. We keep handing them down to student laboratories for research uses. But, by now, even when we have got the equipment, we do not have the money to keep it up in any State.

Another thing I want to mention is that I wish again for some more incentives for industries to give us equipment which they no longer use. That may sound like hand-me-downs, but sometimes there are things that simply are just not used in industrial enterprises.

Chairman FUQUA. You know that was included in a recent tax bill that was passed to try to stimulate that type of gift.

Dr. HOFFMAN. We are beginning to see signs of that. I am beginning to see approaches, as chairman, from people who want to give our chemistry department some type of equipment and I suspected that something of that type was behind it.

Mr. CARNEY. Would the Chairman yield at this point?

Chairman FUQUA. Well, I was going to yield my time to the gentleman.

Mr. CARNEY. I was just going to further pursue that question. The thought that came to my mind is that if we are 10 or 15 years behind in instrumentation in most of our universities, it would be rather difficult to catch up in a hurry, particularly when universities compete with each other for the equipment. Would we be better off if we tried to centralize certain scientific activities at a few universities rather than have it spread out, particularly the highest levels of research that require the very, very expensive types of instrumentation?

Dr. SCHAWLOW. Well, that has already happened in fields like high-energy physics where we have these giant accelerators, but I hope it will not happen in modest-size fields that we are concerned with because we see the horrors of people having to apply to get on a machine maybe 2 years from now, and really good, but unsure ideas not getting any time. You know, ideas that are not sure of success when there are a lot of other things that are perhaps not as exciting but should produce something, do get on. I think it will have to happen with some of the largest machines, but it has grave costs.

Dr. BLOEMBERGEN. May I make an additional comment to that question?

I do not think we should concentrate for the sake of concentration. I think what should happen is ask individual scientists, or small groups of scientists at various institutions, to come up with proposals for equipment upgrading and then award them on the basis of scientific and technical merit. In other words, do not try to distribute them for distribution's sake, nor try to concentrate them for concentration's sake.

Mr. CARNEY. I was thinking along the lines of what has happened in the medical field, not in the research end of it, but where we provide the services in the HSA's. We make a determination that this hospital cannot have this piece of equipment the same identical piece of equipment is in a hospital down the street. Now, that has been a problem in some areas, I recognize that. But, there was an attempt to cut back the cost of giving out, or rendering, high-quality medicine by centralizing that type of thing. I thought it might be applicable here, but it is obvious from one answer that there is a great demand. If you have a 2-year backlog, you would have to distribute to a heck of a lot more than just one or two universities, or five or six, or whatever it is. Wherever they are now, you would have to double the amount I would assume.

Dr. WIESEL. I agree with you that the policies at some times, particularly with some of the expensive equipment, the diagnostic equipment, have been justified. I think that one should here differ between more service-oriented types of things and research. I think the great strength in this country is the diversity. You have laboratories carrying out research, not necessarily big individual laboratories—even in a big university, there are often small laboratories with their own equipment. I think, certainly in biology, a lot of the equipment is now actually being shared. An electron microscope, which costs about $200,000, is often shared between several laboratories nowadays. It used to be bought by individual research facilities, and so on. So, I think that wherever possible people try to create groups with common interest and buy an interest together.

To do it administratively would be a mistake, I think. But, we all realize, I think, that expensive equipment that you cannot use all of the time should be shared by others. I think, certainly in biology and medicine, people are aware of that and try to do so.

Mr. CARNEY. Mr. Chairman, I would simply like, at this point, to thank the gentlemen on the panel and apologize for getting here late, but we had an important committee meeting on oceanography and I am a member of that subcommittee as well.

I would like to say one thing about the—I caught the tail end of the statement of Dr. Hubel, I think,—the scientific exchanges from country to country. It is unfortunate that this has been slowed and I think most people here would support an improvement, but the public does not. Maybe going to see Dan Rather or somebody who runs a national newscasting agency to convince them that it is good for the United States to have that interchange would help.

As a personal observation, I received more flack over a trip overseas, but in those weeks I learned more as a Congressman in Europe than I did in the 3 years I have been here. The trip was not accepted very well by my constituency and you suffer from that same problem. It is unfortunate that it is a perception that we are fighting and it is not here that we are going to win that battle, Mr. Chairman. I yield back the balance of my time.

Chairman FUQUA. Thank you, Mr. Carney. Mr. White?

Mr. WHITE. Thank you very much, Mr. Chairman. We have a tremendously prestigious group here and I have just finished hearing the testimony of Dr. Townes downstairs in the Armed Services Committee, another Nobel Laureate. I would like to expand on the question that was asked, or hinted at, by the chairman, and pose the question to you all as a panel. Suppose we constitute you as an advisory group for the revision of the educational system in the United States, because that is primarily what this Congress is interested in I think. We are so concerned about the shortage of graduating scientists that we feel we are being outnumbered by our allies as well as our adversaries abroad. I would like to ask you as a group and individually what would you do to basically change our education to insure the production and greater numbers of scientists of high quality and to insure that generally our American graduates from high school will be equipped? What can we do to improve the situation and to produce more scientists?

Dr. SCHAWLOW. Let me, since I am closest to you, start first. I do think that the scientists that we are producing who really go the route are pretty good. We have a pretty good university system. Our postgraduate system is as good as you will find anywhere in the world. I think what we are doing is missing a large number of other people who are just totally illiterate in science and a large part of our populus is prey to cults and things like astrology which are pseudo-science because they do not have any basis to judge it upon.

As I mentioned a minute ago, I think part of the problem is that qualified teachers have been siphoned away by better paying jobs, more prestigious jobs, in industry. There was a time when anybody who knew any physics at all could get a better job than he could in high school. Well, the schools do not want to give the science teachers higher salaries than other teachers—I understand that. But, if

we have a special concern for it, Congress would do very well to institute summer sessions which would pay them to pursue their subject matter, either doing some research or taking courses, bringing them up-to-date on recent developments in their subject. This would provide somewhat higher salaries for them because they would have summer pay and it would also increase their effectiveness and that would carry over to the students.

Chairman FUQUA. Dr. Hubel?

Dr. HUBEL. I was just going to say that I think it is a mistake to focus too narrowly on science alone. The average Harvard medical student is practically illiterate. Not that they do not write beautiful prose, but they do not even know where to put the comma or the semicolon. So, if we think that our science teaching is bad, the teaching of what every scientist has to do, namely, communicate by either talking or writing, that is abysmal. My friends here who were educated in Europe probably have had a different time of it but I had to learn how to write, practically from scratch, when I started doing science. I was not quite as badly off because I was educated in Canada, where they teach the Queen's English I suppose one might say, but I do not think that they teach anything in the United States as far as just the rudiments of writing English prose. So, it is a mistake I think to be under the impression that one can turn out people that are educated in science and nothing else. If one wants good education, or better education, I think one has to make it a better proposition for teachers. It is not that our labs are not equipped for teaching high school science and things like that nearly so much as that the best people are not going into teaching.

Dr. SCHAWLOW. I was also educated in Canada and we were indoctrinated to believe that anyone who did not have an Ontario education was seriously deficient. I think we are a bit chauvinistic.

Dr. BLOEMBERGEN. May I comment as somebody who got his high school education in Europe? My native country is the Netherlands. It is quite clear that in those countries there is a great differentiation on the high school level. This, clearly, is not the position in this country and would, obviously, be very difficult to change politically, although there are some high schools that specialize in science, like the Stuyvesant High School in New York City. Certain communities do recognize the importance of striving for excellence at high school age. Maybe, through publicity, we can instill some of that spirit here but one very important thing is not so much the teaching of science in the high schools but the teaching of arithmetic—mathematics—because I think everybody will need more of that to survive in this world of increased reliance on computers and communications. Just as in language, I think that very good instruction in all kinds of mathematics in high school should be encouraged.

Dr. HOFFMAN. I think role models are extremely important. Certainly in my youth, books like "Marie Curie's Biography", "Microbe Hunters" by Paul de Kruif, and "The Biography of George Washington Carver" served very important functions. We do not see anything on commercial TV except doctors, I am sorry to say, and then in circumstances which are not necessarily conducive to attracting people to the medical profession.

I am sorry to say that the only place where we see good role models for scientists is on public television and I mentioned the series "Cosmos" and "NOVA" which are prime examples of such. In fact, I think our country produces some of the best programs in this area, combining the talents of the entertainment industry and, in the case of Cosmos with some good science. I think we should encourage that kind of role model building acknowledging the role of television as a primary medium that reaches our children.

Dr. WIESEL. Not having been educated here and not having had any children going through the educational system, I am not well-prepared. I just know of one community in Massachusetts, Brookline, which is close to Boston, where they have a high local tax base and also the best school system. They have the lowest attendance to private schools in the various communities around Boston because the public schools are very good, but also the tax base is higher than on the other properties. So, it seems to me that in addition to this community, which is a strongly Jewish and Oriental community, they also have the motivation for education.

In addition to motivation, coming back to Dr. Hoffman's comment about what role models to have, we need to put in more money; maybe this should be on a local basis, I do not know, or at the Federal level.

Mr. WHITE. At one time, I had the pleasure of being in Samoa and they had performed an experiment: Because of their money restraints, they had gone to educational television in their school system. They could not afford to pay high-priced teachers. So to get some sort of uniformity and quality in their educational system they used proctors and monitors in their schools, plus the educational television. One could walk into the classroom and the students were so engrossed in watching the television that they never looked up, unlike most other classrooms in which little kids will look around to see who has come in.

Do you feel, considering the breadth of this Nation—the many rural systems that we have, and the city school systems where quality may have suffered because they have not attracted, as you said, the teachers—do you think they should mix a little bit of public television or classroom television of standard courses, basic courses, rolemaking, and things of this nature, to try and upgrade the quality?

Dr. BLOEMBERGEN. I think that would be a very good idea.

Dr. SCHAWLOW. I think we have to realize that we have an opportunity here in the sudden, widespread availability of fairly low-cost videotape recorders; any lecture can be taped for the cost of the tape nowadays. To do them very carefully would take a lot of trouble and you need some way to have some questions, some feedback. It is terribly important for students to learn that what they are told or what they read in books is not necessarily gospel; it is something to be questioned or added to. So, if we go to that, I hope there will be some questioning involved somehow.

Dr. HUBEL. Hearing what you said a few minutes ago, and what Torsten said about the excellence of the schools in Brookline, reminds me of coming to the United States and realizing that if you have children and you move into a place like Boston, you have to look very carefully before you buy your house and decide where to

live. The main thing is how good the schools are. There are these wild differences in schools whereby if you live in Cambridge you have to send your children to a private school if you can afford it, otherwise you do not move there, you go to Brookline.

In Quebec and Ontario where we were brought up, I think there is really nothing quite like that. The best school in Montreal was Montreal High and that was in the poorest community: it is a provincial system. One pays for that, of course, since you do not have the community involvement and you do not have a lot of other features that are very valuable in this country. But, in this country, we pay a terrible price for this wild, nonuniformity of education whereby relocating across the street practically can make the difference between day and night in the quality of school that one goes to. If there could be a kind of uniformity in our education, more than there is, we would certainly have much to gain and, perhaps, some to lose.

Mr. WHITE. Do you think the educational community itself in this Nation, without the involvement of government, which we do not always encourage believe it or not, would be the best ones to go at it, lest you have some favorite project direction from some bureaucrat? In other words: Could the education community in this Nation accomplish what you are really driving at—uniformity and upgrading of quality—without getting a total government program as such?

Dr. WIESEL. Again, I feel very unprepared to answer you, but just to repeat what we said before, you have to have good teachers that are trained well. My intuition would probably be that if you are going to use television and educational material, let the first ones to use them be the teachers. I would probably make it necessary for the teachers to spend 1 month of the summer, not having a vacation but actually upgrading their teaching and that requirement would be part of the thing they would have to do. We would make sure that there were first-rate people involved in upgrading their teaching skills and knowledge and maybe using some devices. I think that introducing television too much in the classroom would be a pity because it is the human relationship with your teacher that you remember: it is the role model, the day to day interaction, and to lose that would be too high a price to pay. I would probably be in favor of upgrading and really giving the teachers a boost both in salary and in terms of motivation to improve their skills.

Mr. WHITE. Thank you, Mr. Chairman.

Chairman FUQUA. Thank you, Mr. White. Let me again offer my sincere appreciation to our Nobel Laureates for taking their time to come and be with us today. I think you have confirmed some of the real concerns that this committee shares, not only this year but in previous years. We have held some recent hearings—series of hearings—on how science and basic research is being affected under budget stress and what the long-range implications for that are. You have touched on the overall concerns we have: instrumentation, the inability of getting bright graduate students to pursue a Ph.D. degree, elementary and secondary education. I want to thank you again for being here this morning. It has been very helpful to us to have the benefit of the wisdom of very distinguished people who not only are we very proud of, but we are proud of the institu-

tions that you represent, and it again shows that where the real research is carried on is in our universities. I want to congratulate each and every one of you and your families for joining us here today, and thank you again for your appearance this morning.

[Whereupon the committee was adjourned at 10:46 a.m.]

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